THE DISTRIBUTION OF INTELLIGENCE

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Current criticisms of the use of the normal curve in assessing intelligence seem to arise from misconceptions both about the nature of the evidence available and about the hypothesis assumed. The theoretical evidence suggests that individual differences in intelligence are the effect partly of multifactorial and partly of unifactorial inheritance: on this basis the distribution to be expected would be bell-shaped, but more or less skewed, i.e. a 'type IV' rather than a 'normal' curve. The empirical evidence is mainly derived from tests scaled independently of the resulting distribution. With such methods, provided the samples are sufficiently large and complete, the frequency-curves obtained exhibit small but significant deviations from strict normality. If, however, specific cases of mental deficiency are omitted, then, for most practical purposes, the resulting distributions can be treated as approximately normal.

I. THE DEFINITION OF INTELLIGENCE

The question which Mr Lewis has examined in his paper on 'the normal distribution of intelligence' (Lewis, 1957) has been raised by several recent writers—particularly those who criticize what they call 'psychometry'.* Dr Heim (1954), for example, has argued that 'frequency distributions on any test depend mainly on the particular system of scoring adopted'; she therefore infers that the notion that intelligence is distributed normally can be nothing but a gratuitous assumption, though 'often hailed as a scientific discovery'. Mr Kenneth Richmond, in an earlier article in this *Journal* (1953), advances a similar contention: 'the psychometrist', he believes, can only ensure the '*a priori* principle' of normality by 'tinkering with the test material'; hence 'measurements appear normally distributed simply because the test has been so constructed that they shall be distributed in that way'. Mr Lewis apparently inclines toward much the same conclusion.

In considering such criticisms it is essential at the outset to make sure that, when they speak of intelligence or of its distribution, both the critics and the writers criticized are referring to the same thing. The word intelligence has such a wide variety of meanings that it is tempting to borrow Spearman's device, and substitute symbols for phrases. Here and elsewhere, when I use it in a technical sense, I intend it merely as a brief and convenient designation for the 'inborn general cognitive factor' (Burt, 1911, p. 95; cf. Hebb, 1949). This fourfold expression seems best to convey what Spencer, Galton and Binet had in mind whenever they employed the term. If, however, we adopt Fisher's convenient practice of using Greek letters for hypothetical variables and Roman for the empirical estimates, then, as I have elsewhere suggested (1955*a*, p. 88), we might avoid much of the ambiguity by labelling this abstract and theoretical concept γ (gamma), in contradistinction to Spearman's g, which is commonly used to denote measurements derived from actual tests.

Nevertheless, each of the semi-popular terms in this familiar definition has at times occasioned some misunderstanding. Hence each calls for a word of explanation. (i) To

* In this paper the word is used in the sense defined by Dr Heim (1954, pp. 1, 22) rather than by Guilford (1936, p. xi); but I do not agree with the sharp division she makes between psychometrists on the one hand and psychologists on the other. A psychometrist who was not first and foremost a psychologist would, in my opinion, be a very bad psychometrist.

those unacquainted with the jargon of factor theory, the term 'factor' is apt to suggest a concrete ability that can be identified by ordinary observation. Thus Dr Heim rightly observes that we cannot 'separate the cognitive factor' from other aspects of mental activity. Yet, though we cannot separate it, we can certainly distinguish it; and for this purpose the factorist employs a method of analysis which is virtually identical with the physicist's method of resolving a resultant force into its hypothetical components. His meaning therefore would be more clearly expressed if he retained Pearson's original term 'component' instead of re-naming it a 'factor'. (ii) The so-called 'general' factor is really a 'group factor', since it is assumed to enter into all measurements of 'cognitive' efficiency, but not into assessments of emotional or temperamental characteristics. As has often been remarked, a truly 'general' factor would embrace something much wider than intelligence, and might be influenced quite as much by motivation as by sheer capacity. (iii) The word 'cognitive' is here used in contradistinction to 'orectic'. But it is often mistakenly supposed to mean purely intellectual activities as contrasted with practical—knowing as distinct from doing. Accordingly, it would perhaps be less ambiguous to introduce yet another technical term, and speak of a 'cybernetic' as contrasted with a 'dynamic' component. (iv) Finally, the word 'inborn' is apt to convey popular and out-of-date notions of 'inheritance', and to suggest that 'hereditary' characteristics can be sharply separated from those that are 'acquired'. The objections urged by Mr Richmond and Dr Heim against this rather naïve conception every geneticist would nowadays endorse. But that of itself does not suffice (as Dr Heim apparently holds) to 'expose the absurdity of such statements as "the (estimated) contribution of innate factors is probably not far from 75 or 80 per cent."'. I myself use such words as 'innate' or 'inborn' merely as short and simple ways of designating 'that part of the variance of the general factor which is attributable to genetic constitution'.

It is, of course, open to the critic to deny that any such component exists, or, if it exists, that it plays any considerable role in determining human behaviour. Elsewhere* I have summarized the vast amount of evidence now available on these points: and, so far as I am aware, no critic has attempted a systematic refutation of the arguments.

There are, however, two alternative interpretations of the term intelligence that are still widely current.

(1) It is frequently urged that what is needed is not a hypothetical but an operational definition; and those who advocate this standpoint commonly adopt the formula that 'intelligence is the mental characteristic measured by intelligence tests' (e.g. Fraser, 1954 p. 11). Intelligence as thus defined I have called g_t: it corresponds fairly closely with Spearman's assessments of g, which were based, not on factor analysis, but on certain tests selected as 'reference values'. Apparently this is the definition on which Mr Lewis relies in the latter part of his paper. In practice, however, to define intelligence in this circular manner is as unhelpful and as uninformative as it would be to define temperature as 'the physical characteristic that thermometers measure'. To begin with, how are we to determine what can be accepted as tests of intelligence and what cannot? Mr Lewis seems to regard the Moray House tests as the best and most typical; others would take the Terman-Merrill tests as their standard; others again prefer tests of a non-verbal or performance type. Furthermore, all such tests have imperfect correlation

* The definitions given above have been more fully explained in earlier publications on the subject (1911, 1912, 1917, 1921) and the evidence reviewed afresh in several recent papers (1943, 1955, and refs.).

both with each other and with independent assessments. Thus, as with other forms of psychological measurement, their results are largely affected by incidental influences irrelevant to our main purpose, and are consequently disturbed, to a far greater extent than physical measurements, by error. But if we have no antecedent definition of what we want to measure, how can we distinguish what is irrelevant and what is not?

(2) The majority of the more recent critics, however, would like to get right away from the notion that intelligence is to be defined in technical or quantitative terms. Dr Heim, for instance, complains that the postulate adopted by the statistical psychologist—that 'there exists some one attribute, "intelligence", which is one-dimensional, measurable, capable of quantitative but not qualitative differences'—is far too remote from the 'ordinary meaning of the word as the layman understands it', and that 'to take a popular word, denude and disguise it and try to restrict its circulation', is 'quite indefensible'. Similarly, Mr Richmond describes the concept of 'measurable intelligence' as an *ignis fatuus* which leads the student into the region of statistical jargon where he 'soon finds himself bogged down in mathematical abstruseness'. The proper procedure, we are told, is to assign the word 'a meaning that is comfortably compatible with that of the layman'. Thus, when Dr Heim and Mr Richmond speak of 'intelligence', they treat it as a synonym for what I dubbed g_i (Burt, 1955*a*, p. 86).

In point of fact, however, 'intelligence' is not a word of popular origin. It was coined as a technical term to express a highly technical concept. Like other scientific or philosophical expressions, it has been borrowed by lay writers, and used so loosely as to be almost worthless for purposes of exact logical argument. But why should the layman dictate to the scientist how his terms are to be defined? No one would think of telling the chemist that his definition of a gas should be governed by the way the word is used in common parlance, or argue that, because the kinetic theory of gases can only be expressed in 'statistical jargon' and with 'mathematical abstruseness', it should be discarded in favour of something 'comfortably compatible' with the untrained layman's comprehension.*

But, whether we accept these linguistic criticisms or not, they do not of themselves rebut the psychometrist's contention that intelligence *as he defines it* is distributed in approximate conformity with the normal curve. Dr Heim and those who follow her seem to have mistaken a disagreement over names for a disagreement over facts. No psychometrist, I imagine, would dispute her statements that 'men differ qualitatively as well as quantitatively', or that they vary in the traits she calls 'social intelligence', 'creative intelligence', 'verbal intelligence', and the like: what he objects to is the practice of lumping together a number of diverse and complex characteristics—some involving highly specific abilities, others involving temperamental as well as cognitive components—under one and the same term 'intelligence'.

Mr Lewis's attack on the normal curve goes much deeper. He does not, if I understand him rightly, quarrel with the psychometrist's definition. On the contrary, accepting that definition (or something very near it), he adduces several arguments to show that the claim that intelligence is normally distributed (*a*) is 'by no means self-evident',

* Since the above was written Mr T. R. Miles has published the abstract of an admirable paper 'On Defining Intelligence' (*Brit. Psychol. Soc. Bull.* (1956), **29**, 2), in which he criticizes (amongst others) these last two definitions from the standpoint of modern logic. I should, I think, readily assent to all the objections he has advanced.

(*b*) has not been 'established from the results of intelligence tests', and (*c*) cannot be 'deduced from genetic grounds'. In the end he too comes to much the same conclusion as Dr Heim, who holds that the hypothesis of normal distribution is supported by no evidence or proof, and is therefore at best merely a convenient fiction and at worst an unwarrantable distortion of the original scores. The psychometrist postulates normality when standardizing his tests; and therefore the 'quotients' secured with such tests conform with the normal curve, but that is merely an artificial consequence of the assumption he has arbitrarily made.

Now I venture to think that all this gives the reader a rather unfair picture of the type of proof on which those who are criticized by Mr Lewis and Dr Heim have actually relied. Like other generalizations in natural science, our conclusions about the nature and distribution of intelligence rest on what Whewell has called 'a consilience of inductions'. They are supported, not by just one kind of evidence (such as 'the distribution of quotients'), but by several converging lines of argument. Being inductive, each inference by itself can lead to no more than a probable conclusion, and its probability, considered in isolation, may be relatively low; but the *cumulative* effect of the different arguments is overwhelming.

The effect of 'consilience' is so persistently overlooked that it is worth while emphasizing its importance. Let us suppose that (as argued below) there are five hypothetical curves that deserve empirical investigation, and that the antecedent probability (based on genetic theory, and the like) in favour of the second of these (a moderately asymmetrical curve of type IV) is twice as great as that for any other single type: this would evidently give it an *a priori* probability of 2 out of 6, i.e. 1 out of 3—a very modest estimate in view of the relative infrequency of the other curves in previous genetic work. And now let us imagine that, in general, the odds in favour of a verdict given by any one of the independent sources of empirical evidence described are 4 to 3—once again an extremely conservative estimate. There are three such sources. Accordingly, on applying the usual formula, it can readily be shown that, if all three sources of evidence agree in confirming the second hypothesis, the *a posteriori* probability that this hypothesis is correct will be 0·97: in other words, the odds in favour of that hypothesis have risen from 1 out of 3 to 31 out of 32 (for the method see Keynes, 1921, pp. 182f.). Of course, no one pretends that such probabilities can be exactly assessed, or that such formulae can be regarded as anything but simplified approximations: but they serve to bring home in a striking way the cumulative force of converging lines of evidence.

It is all too easy for a critic to seize on one particular argument and to ignore the fact that the reasoning is avowedly based on a comparison of probabilities: he can then demonstrate that this one argument is inconclusive, i.e. that it can claim the certainty neither of a 'discovery' nor of a 'self-evident proposition', nor yet of a formal 'deduction'. But plainly this is, in the most literal sense, an *ignoratio elenchi*. What the critic has to do is not simply to demonstrate that a probable conclusion cannot claim the certainty of a deduction or a self-evident postulate, but rather to show that *his* hypothesis has a higher probability than the one which he is criticizing; and this can best be done by adducing fresh empirical data from an appropriately planned inquiry of his own.

II. THE ALTERNATIVE HYPOTHESES

The object of our inquiry is to determine the distribution of *innate* ability. We can admit straight away one point repeatedly stressed by the critics—namely, that the results of every test, taken as they stand, are bound to be affected by environmental and other post-natal influences. What I myself have tried to show is that at least 75% of the total variance must still be due to genetic influences, and probably far more (1955*b*, p. 175 and refs.); and this should be sufficient for our purpose.

Dr Heim, however, tells us that her first aim is to expose the absurdity of all such statements (1954, p. 124). Her argument is that any attempt to estimate the relative contributions of 'nature' and 'nurture' 'must imply a separation which does not exist'. But her contention, if valid, would manifestly undermine almost every attempt at biometric analysis: for, as the geneticist constantly reminds us, '*every* character can—and in fact nearly always does—show heritable and non-inheritable variation simultaneously' (Darlington & Mather, 1949, p. 64). Her difficulties, I fancy, really arise from a confusion between the individual and the population. The analysis that she proposes to refute is really an analysis of the variance of a *population*. Such an analysis, she says, 'taken to its logical conclusion leads to such absurdities as the following'; and she then gives two imaginary analyses of an *individual's* deviation ('Tommy's') from the parental mean in terms of the contributions of heredity and environment respectively. But this is *not* a logical conclusion; it is a logical fallacy—a fallacy so common that Aristotle and his followers gave it a special name, *παρὰ τὴν διαίρεσιν*. It is like arguing that because 15% of the income of the *population* is unearned, therefore 15% of *Tommy's* income must be unearned.

Fisher has succeeded in deriving (on the basis of certain reasonable conditions) perfectly valid formulae for analysing the variance of a population; and in the case of stature and other physical characteristics has obtained plausible estimates for the relative contributions to that variance of (*a*) 'heredity' (defined as including 'genotypes', 'dominance', and 'homogamy') and (*b*) 'environment' ('using that term formally for causes independent of heredity') (Fisher, 1918, pp. 422–4). It is difficult to see why arguments and procedures used in the case of physical measurements cannot also be applied in the case of mental measurements. In both cases we have to allow for minor errors of measurement and for the effects of environment (defined as above); and in the case of mental measurements these two contributory factors will no doubt be somewhat larger. But I can discern no difference of principle.

Dr Heim and Mr Lewis would apparently condemn all such methods as invalid. Yet they never explain where precisely Fisher, and those who follow him, go wrong. And, since Fisher's general views are accepted by practically all geneticists, it is surely permissible, until a more direct refutation is offered, to accept them here.

However, although we may now agree that the same kinds of genetic mechanisms determine both physical and mental variations, the detailed processes involved may of course differ appreciably in the case of this particular characteristic or that. What then are the alternatives that we have to consider in the case of intelligence, and what types of distribution are they likely to produce? Within the framework of current genetical theories there are, I think, four distinct hypotheses that deserve to be examined: and each leads to its own distinctive form of frequency-curve.

(1) *Symmetrical and normal.* The simplest hypothesis would be to assume that differences in intelligence are due solely to multifactor inheritance, and to treat the effects of subsidiary processes—dominance, linkage, assortative mating, and the like—as too slight to affect the ultimate results. On this basis, as Karl Pearson showed (1903), we should expect a perfectly normal distribution. This simplified mechanism is the one which Mr Lewis apparently supposes that the psychometrist postulates. (In his paper, as printed, he now agrees that 'this is undoubtedly an oversimplified mechanism'.) But this rather primitive picture disregards a number of well-established facts, and leads to correlations between relatives far lower than those actually observed (Burt, 1955*b*, p. 174). Certain types of extreme deviation seem undoubtedly due to single genes; and in some types the responsible gene is dominant, in others recessive, and in others again sex-linked (Burt, 1935; Fraser Roberts, 1940). In the clearest cases the deviation appears to be subnormal and well marked; but there is evidence, far less conclusive, to suggest that occasionally the deviations so transmitted may be relatively slight, and in others perhaps supernormal. Moreover, the resemblance between parents due to assortative mating can hardly be less than 0·40. There can be little doubt, therefore, that the conditions to be postulated are not those adopted by Pearson, but the far more general conditions assumed by Fisher in his later inquiry into the mathematical consequences of the Mendelian theory (1918).

(2) *Moderately asymmetrical.* The hypothesis that I myself tentatively put forward (e.g. 1935, pp. 79f) was that, for intelligence as for height, weight, and many other graded characteristics, we seem compelled to postulate two modes of inheritance: (i) in certain cases, as the pedigrees indicate, the deviation studied may act like a recognizable trait dependent on a *single* gene—probably in most cases partially dominant, and sometimes sex-linked; (ii) in other cases it is apparently determined by the joint action of a *large number* of genes. The 'major genes' seem comparatively rare, but each will produce effects that are large and (it would appear) for the most part detrimental; the 'polygenes' must be much more numerous, but their effects will be too small to be identified individually. With this double assumption, the resulting distribution would take the form, not of the normal curve, but of an asymmetrical, bell-shaped curve of unlimited range in either direction. To borrow Pearson's convenient nomenclature (1914, p. lxiii), it would be 'type IV' rather than 'type G' (the 'normal' or Gaussian, which is in fact a limiting form to type IV). Thus, as a result of this twofold method of transmission, 'the inheritance of intelligence would resemble that of stature, though with a more marked divergence from strict normality' (Burt, 1935, p. 81).

(3) *Highly asymmetrical: negatively skewed.* The foregoing are by no means the only hypotheses that demand consideration. Several writers have insisted that genetically intelligence must be an exceedingly complex quality (a view which I should not for a moment deny—so indeed is stature); and from this they infer that 'an exceptionally high degree of intelligence must be the resultant of exceptionally large deviations, each exceptionally rare, affecting numerous special abilities'; these favourable conjunctions, they consider, must be 'decidedly infrequent'. Hence 'the number of persons superior to the mode must be much smaller than the number of those inferior'. Such a theory would produce a distribution with a pronounced degree of negative asymmetry, since the upper tail would be markedly compressed (cf. Snyder, 1940, ch. xiv).

(4) *Highly asymmetrical: positively skewed.* Finally, one or two writers have suggested

hat the effects of the genes 'might combine by multiplication instead of by addition'. n that case differences in net ability would correspond more closely with differences in utput, and so, indirectly, with differences in income. On these and other grounds, Pareto nd his American followers have inferred that the distribution of innate ability must e highly asymmetrical and possess an 'elongated upper tail'. Thus, as Pareto himself bserves, the highly skewed curves for the distribution of income are 'not a secondary onsequence of *economic* conditions, but reflect a *biological* law' (cf. Davies, 1941, pp. 427 f.).

(5) *Other types.* Mr Richmond thinks that 'the true distribution of intelligence may ell follow a curve of its own'. But neither he nor Dr Heim indicates what particular ype of curve they have in mind. Mr Lewis, it would seem, is inclined to believe that its orm would be approximately 'rectangular'. But he offers no evidence from genetics o show how this could occur.

Had we no empirical data bearing directly on the distribution of intelligence, we should, think, assign the greatest antecedent probability to the *second* of the foregoing hypoheses, since, as Pearson himself (1895) has pointed out, the prevailing curve in zoological nd anthropological material is type IV with 'a moderate degree of skew variation'. his is the curve I myself originally assumed (1917, p. 34; cf. 1937, p. 442 and refs.).

III. THE EMPIRICAL EVIDENCE

et us now turn to the objective evidence furnished by first-hand surveys and the like, nd inquire how far it tends either to confirm or to confute these rival conjectures. As Ir Lewis observes, the practical experience of school teachers and education officials trongly suggests 'a humped or bell-shaped distribution', but throws no light on the prob-em of symmetry. 'Most important of all', therefore, in his view, 'is the claim that the ormal distribution of intelligence has been established from the results of intelligence ests: e.g., Burt, 1953'. As worded, this statement may perhaps be a little misleading.

To begin with, the data on which I myself have principally relied consist, not so much f 'raw scores' or 'quotients derived from the Binet scale' and taken at their face value, ut rather of corrected assessments based on all the information available. What we are oping to determine is the distribution of *innate* ability; and the results of a single test f intelligence applied on a single occasion are too much affected by environmental onditions to be accepted for this purpose just as they stand. It has therefore been my egular practice to submit to the teachers in the schools concerned a list of marks in order f merit for pupils in their charge, and to ask them to criticize the actual scores; then, whenever any marked discrepancy has emerged, the child has been re-examined, usually vith a different type of test (1943, p. 98).

Secondly, my 'claim' was not to have 'established the normal distribution of intelli-ence', but rather the contrary. From 1917 onwards the normality of the frequency-istributions obtained from my L.C.C. surveys was regularly checked by the χ^2 method cf. Burt, 1917, p. 34); and in every case the distribution was found to diverge signifi-antly from the normal. Certainly, for many practical purposes the distribution of pupils n the ordinary schools (i.e. excluding specific cases of deficiency) might, as I pointed out, e treated as '*approximately* normal, but only *approximately*'; and to avoid misunder-tanding I italicized the word (1943, p. 92).

However, if I understand them aright, Mr Lewis, Mr Richmond, and Dr Heim would leny even a tendency towards approximate normality. Any such approximation, they

hold, is a sheer artefact—a by-product of the arbitrary way in which the tests have been standardized. Similarly, Mr Richmond, as we have already seen, declares that the psychometrist can only secure this appearance of normality by tinkering with the material that his tests supply; and 'this rigging of the test-situation', he insists, 'is as false as it is artificial' (1953, pp. 227f.).

Mr Richmond makes these assertions dogmatically without explicit evidence. But Mr Lewis goes further: he attempts to demonstrate them. His argument is a little difficult to follow, because, instead of offering a general algebraic proof, he deduces his conclusions from five artificially constructed examples. For each he chooses a test with six items and a sample of six individuals.* But with such small numbers it is impossible to prove any general proposition about frequency distributions. *With the particular numbers he selects*, Mr Lewis is able to show that, when the items are all of equal difficulty ($F.v. = \frac{1}{2}$), 'central concentration' (the nearest he can get to approximate normality) can only occur when the validity coefficients (E_{13}) range from 0 to 1; if, however, all or nearly all the items have perfect validity ($E_{13} = 1$), then there will be 'a tendency towards rectangularity rather than normality'. Now, it is quite easy to demonstrate that, with a different number of items or a different number of testees, precisely the opposite may occur. Suppose the test contains seven items instead of six, then, with the conditions Mr Lewis specifies for a 'tendency towards normality' (scheme 1, p. 100), we obtain the following:

Successful groups	*a b c*	*a b d*	*a b d*	*a b e*	*a c d*	*a c e*	*b c f*
F.v.	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$
E_{13}	1	1	1	$\frac{1}{2}$	$\frac{1}{2}$	0	0

Frequency distribution

f	*e*	*d*	*c*	*b*	*a*
1	2	3	4	5	6

Here, instead of a 'central concentration', we now get a specimen of 'rectangular' distribution quite as perfect as any Mr Lewis can produce with the conditions which he himself prescribes (scheme iv, p. 101).

But I need not labour these objections, since no statistical psychologist, I fancy, would wish to dispute Mr Lewis's final conclusion: namely, that the frequency-distribution obtained from a composite test can be forced into almost any shape or form by suitably selecting (or scaling) the constituent test-items, provided the selection (or scaling) is based *on the same sample of persons*. It is merely his proof, and his formulation of the methods needed to secure this or that type of curve, that are incorrect (for a more rigorous approach to the same problem, see Lord, 1952).

It seems clear, however, that such methods will no longer serve when the individuals or groups used for the final distribution are drawn from a type of population *different* from that which is used for selecting or scaling the items. This will become obvious if we consider Mr Lewis's fourth scheme (p. 101). Here we might imagine his six groups, *a*, *b*, . . . , *f*, to be six consecutive age-groups, and his six items, 1, 2, . . . , 6, to be six ideal questions of the Binet-Simon type, each a little harder than the last and each passed by the same percentage of children of the age for which it is designed (Mr Lewis assumes 100%, which would be impossible in practice; I should prefer 50%). Then, as his scheme implies, the average mark obtained by the individuals in each *successive* age-group would

* Mr Lewis calls them 'equal-sized groups', and assumes that each member in any one group responds identically throughout the series—an assumption which could never be fulfilled unless the testees were as rigorously selected as the items.

yield a rectangular distribution. That of course is true of the actual Binet tests. But his argument in no way demonstrates that the intelligence of the individuals within the *same* age-group is distributed in rectangular fashion; it might just as well conform to some bell-shaped curve, possibly normal, possibly not: we cannot possibly tell until we have examined the actual scores.

In point of fact, as Mr Lewis implies, he is really thinking of the methods adopted for selecting and evaluating test-items in a *group* test such as the Moray House. Now the purpose of such tests is entirely practical: they were never intended to determine the theoretical distribution of intelligence, but merely to enable the users to allocate pupils of one particular age to the most suitable school or class. Moreover, unlike the Binet tests, they are standardized solely on children of the age for which they are to be used. Quite illogically, however, having shown that the distribution of measurements from Moray House tests is 'imposed on the scores during the process of standardization', Mr Lewis goes on to infer that the distributions obtained with the Binet tests will similarly depend on the methods of selecting and scaling the constituent items: 'its bell-shaped distribution of quotients, no less than the roughly rectangular distribution of Moray House Tests, may be regarded as a consequence of the "facility" and of the E_{13} values of the component items'. But, since the Binet scale is standardized in an entirely different fashion, this is a complete *non sequitur*.

(i) *Scaling by mental age.* Mr Lewis deals with the method of selecting items for the Binet scale in another section of his paper. Binet, we are told, allocated a test-item to a given age 'if it could be passed by about two-thirds of the testees *of that age*: Burt (1921), in his adaptation of the Scale, adopted instead the ratio one-half'. This implies that, for Binet's ratio of two-thirds of the specific age-group, I myself merely substituted the ratio of one-half of the *same age-group*. But that is incorrect. My proposal was based on an argument quite different from Binet's. The guiding idea was that the age level assigned to any item could be regarded as a 'threshold' or 'limen' (in the sense of the theory of psychophysical methods). The working procedure was laid down as follows: 'Let the children be grouped, not, as with Binet, by the age at their nearest birthday, but by their age at their *last* birthday. The age of the groups will thus average $5\frac{1}{2}$ years, $6\frac{1}{2}$ years,..., respectively, not 5·0 years, 6·0 years,.... Now a correct allotment of the test implies that in every group the average mental age shall coincide with the average chronological age. Hence to be assigned to any given year (as a teacher would interpret a mental year*), a test should be passed by approximately 50% of the children of that age.' In short, the measurement 'x mental years' means the performance of a *median* child of x chronological years (Burt, 1921, pp. 140 and 271).†

* If, for example, *all* the children aged 5·0 to 6·0 pass all the tests for age 4·0 to 5·0 and *half* the tests for age 5·0 to 6·0, both their chronological and their mental age will average $5\frac{1}{2}$: from this it is clear that the same requirement will be fulfilled if we suppose that *half* the children pass *all* the tests for age 5·0 to 6·0

† The accuracy of this principle seems to have been endorsed by most later psychometrists. Guilford (1936, p. 210–1) discusses the appropriate procedure for Binet tests in his chapter on 'Constant Methods'. 'In establishing the age levels of his various tests, Binet' (he writes) 'adopted a rough and ready standard: any test that could be passed by from two-thirds to three-fourths of the children of a certain age should be placed at that age level. This means that the average child can do *more* than pass the tests for his age. This is a violation of the use of medians and means. A five-year test should be one that the average five-year-old child can pass, or one that 50% of them can pass.' For the Terman–Merrill revision, a similar principle was subsequently adopted (Terman and Merrill, 1937, p. 9). For the original Stanford Revision a rather different procedure was used.

Now how can the use of medians for *successive* ages affect the distribution of mental ages for individual children *within* a single year? No assumption of normality is made; and the fact that the distribution actually secured fails to conform with the normal clearly proves that the mode of selection does not necessarily entail normality. All that is assumed in the process of scaling is that the intervals of difficulty from one mental age to another may be treated as equal.

To illustrate the type of distribution obtained, I give the results of one of the most thorough of the London surveys (Fig. 1). The figures are based on the results of the English Revision of the Stanford–Binet Scale, checked by the teachers; the children tested, 2835 in number, formed an unselected sample of the general school population between the ages of 6 and 11. The data were collected in the course of a joint research on the standardization of the Stanford–Binet Scale for English children (Burt, 1953, p. 348). I must again express my gratitude both to my co-workers and to the head-teachers concerned for their willing collaboration. The survey was organized as part of my regular duties for the L.C.C.

In terms of the I.Q., the mean is 100·3, the median 101·0, and the standard deviation 14·9 (15·2 for the lower half, 14·7 for the upper). The dotted line shows the best-fitting normal curve. On applying Pearson's test for goodness of fit, we obtain $\chi^2 = 59{\cdot}1$, and $P = 0{\cdot}000001$; i.e. it is about a million to one against the sample being drawn from a normally distributed population. The functions required for determining the form of the curve (cf. Pearson, 1914, p. lxiii) are $\beta_1 = 0{\cdot}035 \pm 0{\cdot}022$, $\beta_2 = 3{\cdot}36 \pm 0{\cdot}18$, $\kappa = 0{\cdot}04$; this implies that the curve is of type IV. The skewness ($-0{\cdot}079 \pm 0{\cdot}024$) is small but statistically significant; the leptokurtosis less marked but still fully significant.* If from the distribution here depicted specific cases of mental deficiency are omitted, χ^2 drops to 26·7 and P rises to 0·044. Thus the deviation from normality still remains statistically significant, though it would only be demonstrable with fairly large samples.

Dr. Heim and Mr Lewis, as we have seen, attribute all appearances of normality to the skill of the test constructor; but other critics may be inclined to wonder whether *deviations* from normality, such as we discover here, are not merely the incidental consequences of his *lack* of skill. This interpretation might seem plausible had we had no more than one or two sets of data to account for. But the numerous surveys carried out by various investigators nearly all yield constants of much the same order, generally tending in the same direction. As I have pointed out elsewhere, 'for most of the distributions β_1 lies between 0·0 and 0·2, and β_2 between 2 and 4', so that the skewness usually lies

* It is instructive to compare this with the corresponding values for bodily measurements. For male height the British Association figures (1883) give $\beta_1 = 0{\cdot}0002$, $\beta_2 = 3{\cdot}15$, $sk = -0{\cdot}01$, but probably many dwarfs were omitted from the survey; for weight, $\beta_1 = 0{\cdot}54$, $\beta_2 = 4{\cdot}46$, $sk = +0{\cdot}29$ (Yule & Kendall, 1937, pp. 161, 546). Very similar beta-functions are given for skeletal age (eleven age-groups) by Dearborn & Rothney (1941, p. 103). In testing normality I myself should prefer to use estimates of the gamma-functions, as suggested by Fisher (1934, pp. 53, 77); but for bodily measurements the beta-functions have hitherto almost invariably been adopted. As I have pointed out elsewhere, with large and unselected samples the most suitable equations for fitting a type I curve to data for intelligence seem to be those for the hypergeometric series. The normal curve provides a limiting case; its equations are therefore simpler, and will yield reasonable approximations, except where frequencies for markedly subnormal or supernormal children are required: for such satisfactory estimates may be obtained most simply by assuming that the curve consists of two normal halves with slightly different standard deviations. These devices have been so often used in practice (cf. Burt, 1943, p. 85 and refs.) that it is surprising Mr Lewis makes no reference to them.

With tests of attainments, the distributions obtained are of a rather different type (cf. Lord, 1954).

etween 0·05 and −0·10 (Burt, 1943, p. 93). This general agreement indicates some more undamental cause, such as is suggested by the second of the four hypotheses mentioned bove.

Even where the deviations from normality are not fully significant, they are still of the ame nature as those described above. One of the largest surveys of unselected children s that carried out by Dr Fraser Roberts and his colleagues (1935, pp. 334f., 'Form of Distribution of Scores'). Practically every child in the city of Bath between the ages of and 13—2553 in all—was tested with the Advanced Otis Scale. With the χ^2 test, the odds gainst the data being drawn from a normally distributed sample were about 6 to 1. The divergence therefore is not fully significant. But there is a definite 'heaping up at ne lower end of the scale'; and this, as the authors observe, is 'typical of the results of ntelligence testing' (cf. also Fraser Roberts, 1940, pp. 237f.).

However, the data supplied by the Binet tests is only part of the evidence available for xamination. Tests scaled in entirely different ways point, as we shall see, to much the ame conclusion.

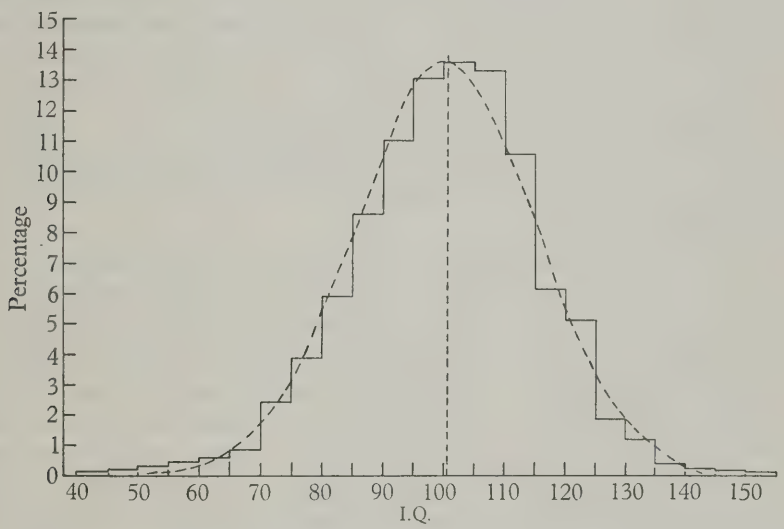

Fig. 1. Distribution of intelligence. The dotted curve shows the best fitting normal distribution.

(ii) *Scaling by just noticeable intervals or equal appearing intervals.* In the early days of ntelligence testing, it seemed doubtful how far we were justified in assuming that the ifference between one mental age and the next was the same throughout the range oncerned. I myself (1914, 1921) believed that with the Binet Scale the intervals showed nly a very rough equality. Accordingly, alternative methods of scaling were tried—most f them based on the traditional psychophysical procedures. For instance, in constructing test of Opposites, Mr Moore and I arranged a large number of items in order of difficulty, nd then submitted successive pairs to twenty teachers, with the following instruction: Will you compare the difficulty of these two items, and say whether you consider the econd to be (i) harder, (ii) easier, or (iii) of about equal difficulty?' Whenever approxiately 50% of the judges agreed that the second was harder than the first, we regarded ne difference as 'a just perceptible increase'; and on this basis we picked out a series in

which the increment of difficulty exhibited by each item over its predecessor was 'jus perceptible' in the sense defined. Where fewer items were available or needed (e.g. wit Completion tests), we graded them by employing instead the method of 'equal appearin intervals'. On applying tests so standardized to random samples of children, the dis tributions obtained were always approximately normal, though never exactly so Similar results were reported in other countries (cf. the curves printed by Trabue, 191 figs. 10–14: for the psychophysical methods, v. Brown & Thomson, 1925, pp. 46f).

(iii) *Scaling by number of unit-processes performed.* All the foregoing are 'graded' tests In what may be called 'homogeneous' tests the constituent tasks are all of approximatel equal difficulty. With these the examinee's score is expressed in terms of the number o unit processes correctly performed within a given time limit.

There are numerous varieties. Before the current type of group tests attained thei present vogue, one of the most popular devices was the so-called erasure or cancellatio test. The examinee is given a sheet of pied print, and is asked to cross out one or mor letters (A, E, O, for instance) wherever they occur: the measure is simply the numbe of letters correctly cancelled. These and other tests, scored on a similar basis, were freel used by earlier investigators; and frequency distributions may be found in their publica tions or unpublished theses:* graphs for the commoner types were printed in Thorndike' first book on mental measurement (1904, figs. 30, 31 32, and 34; each diagram shows normal curve fitted to the distribution). As Thorndike points out, except when th nature of the scale is likely to introduce distortion (e.g. in measurements of reactio time), practically every distribution 'approximates to the so-called normal shape, i.e that of the probability curve'.

Thus, with each of the different methods of scaling I have described, the resultin distributions have much the same form. Nevertheless, none of these scaling method would be accepted as final by the geneticist without a further test. For him a scale o measurement is satisfactory only if the effects of each and all the various causativ agencies—whether genes or non-inheritable factors—are demonstrably additive (cf. Fishe *et al.* 1932; Sewall Wright, 1952). This of course is an ideal; and often we have to b satisfied with a scale for which the effects are additive *on the average.* In dealing wit animals this can be checked in various ways: e.g. if we cross two true-breeding lin (P_1 and P_2 say) and then backcross the first generation (F_1) to one or other of the paren groups, we shall obtain means for five groups, $\overline{P}_1$, $\overline{B}_1$, $\overline{F}_1$, $\overline{B}_2$, $\overline{F}_2$; and, if the scaling additive, these should be equidistant; if the contributions to the variances are also add tive, the variances of the true-breeding lines and of the first generation should be th same, even when the means differ (for an admirable application of this procedure see Cha 1956). With physical measurements, so it appears, the natural unit fulfils these requir ments well enough in the majority of cases; but occasionally (e.g. with body weight o volume) the original measurements need to be re-scaled by adopting a logarithmic o similar transformation. With psychological measurements and with human beings seems possible that rough tests along analogous lines might be devised by selectin appropriate pedigrees. For intelligence, as we have just seen, much the same type distribution is reached with several independent methods of scaling. Hence it seems fa

* A description of tests of this type which had the highest validity will be found in a joint paper by Mr R. Moore and myself (1912): frequency distributions were given in Mr Moore's thesis.

o infer that here too what may be regarded as 'natural units' are adequate for the broader ssues with which we have been concerned.*

However, the study of frequency distributions forms by no means the only source of vidence available for verifying the hypothesis proposed: the coefficients of correlation, the ivariate distributions for parents and their offspring, and for children and their brothers nd sisters, all tend to confirm the same theory; and the other statistical constants all losely agree with the theoretical values deducible from the hypotheses outlined above Burt, 1935). In the revised version of his article, Mr Lewis briefly refers to the paper n which these further arguments are set forth; but he makes no attempt to rebut them.

Finally, there is the more practical test. The assumption of approximate normality as now been adopted for a wide variety of administrative purposes over a long period of ears. In organizing classes, in planning the number of schools to be built or allocated, in evising or standardizing schemes of marking or grading, and in numerous other ways, he scheme of proportional frequencies suggested by the normal curve (e.g. those of the .C.C. *Report*, Burt, 1917) has proved both serviceable and reliable, despite all the objec- ions urged by the earlier sceptics forty years ago. It is difficult to understand how this ould have happened if the foundations on which such schemes are based were as faulty s the modern critic avers.

IV. Summary and conclusions

n current controversies about the distribution of intelligence, the term intelligence is nterpreted in very different ways. In this paper it is used in the technical sense given to , explicitly or implicitly, in the work of Spencer, Galton, Binet, and their followers, amely, 'the innate general factor underlying all cognitive activities'.

Four main hypotheses have been advanced to account for the manner in which it is istributed; and the frequency-curves deduced from each of the hypotheses prove to be

* Space will not permit me to reply to Mr Lewis's criticisms of the I.Q. as a unit of scale. He quotes Dr anks and myself as 'claiming that I.Q.'s should be interpreted as measurements on an interval scale, so that the fferences between I.Q.'s may be legitimately compared', and making the 'still more ambitious claim that with hurstone's technique the measurements may be interpreted as measurements on a "ratio scale"'. 'The asis of these interpretations' (he adds) 'is the normal distribution'. Now, if the reader will turn to the page r Lewis cites (Burt, 1953, p. 161), he will see that I.Q.'s and Thurstone's scale were inserted in our table merely to *lustrate* the concepts of an 'interval scale' and a 'ratio scale' in psychology: not a word was said about 'claims', about the 'legitimacy' of such procedures, or about the normal distribution as a 'basis'. On the contrary, ie point of the table was to show that measurement in psychology need *not* necessarily take either of these two rms. Mr Lewis's argument in the paragraph which precedes these criticisms—namely, that those critics who ppose that all psychological measurement must be based either on an 'interval scale' or a 'ratio scale' have erlooked the use and fundamental nature of the 'ordinal scale'—is precisely the point that we ourselves were ncerned to make (*loc. cit.*, last paragraph).

Dr Heim similarly criticizes psychometrists for their 'universal' and 'unhesitating' use of the I.Q., and re- mmends a percentile basis instead. But percentiles (or S.D.'s derived from percentiles) were in fact what I yself had recommended and used for more technical inquiries (cf. *The Backward Child*, pp. 20–24 and refs.); nd I have repeatedly stated that I do *not* consider the I.Q. to be the best unit for scientific purposes (e.g. 1917, 15). However, these more elaborate procedures have been assailed by critics on the opposite flank—those ho dislike all 'tinkering' with the initial data. For the graph printed here, therefore, I have retained the I.Q. being at once the most direct and the most intelligible unit. But I should emphasize that I do not consider ie units, the mean, or the standard deviation, used to compute the smoothed curve in this particular survey, represent the most appropriate measurements for other problems besides that examined here (cf. Burt, 937, p. 442). And, should any reader feel that they may be equally inappropriate for the present problem, may point out that the majority of the graphs and frequency-curves cited above were *not* based on the I.Q.?

sufficiently distinctive to furnish a crucial test. Of the four rival theories that which, o genetic grounds, has the strongest *a priori* arguments in its favour assumes that intelli gence, like most other graded characteristics, is the result of both multifactor an unifactor inheritance. This implies as a corollary that the curve of distribution will b bell-shaped and moderately asymmetrical: i.e., 'type IV' rather than the 'normal' type

The empirical evidence is derived principally, though not entirely, from the results o intelligence tests. Except for those intended solely for a single age-group, most tests ar scaled by methods independent of any presupposition about the resulting distribution e.g. (i) by mental age, (ii) by least perceptible or equal-appearing intervals, or (iii) b the number of unit processes performed. With each of these procedures the frequency curves obtained yield, in nearly every case, statistical constants implying a definit though slight degree of leptokurtosis and negative asymmetry. Thus the results are fully consistent with the twofold hypothesis suggested by genetic arguments, and entirel incompatible with the other three hypotheses.

If children certified as mentally deficient under the Acts of 1913 and 1921 are omitted— a procedure which eliminates most of the cases due to unifactor inheritance—then th divergence from normality is comparatively slight, and with small samples ceases, a a rule, to be statistically significant. However, in computing the frequencies of well marked deviations, whether in the direction of high ability or low, any assumption o strict normality should be used with the utmost caution.

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Note on the 'Index of Item Validity'. Presumably because he believes that intelligence follows a rectangular distribution, Mr Lewis (loc. cit., p. 99) has preferred to assess item validity by a point biserial correlation instead of the more usual tetrachoric correlation. In that case with three equal subgroups the equation should be (in his notation)

$$E_{13}=\frac{3(S_1-S_3)}{n}$$

This is the formula I have used for the table on p. 168. Mr Lewis's equation as printed substitutes a multiplication sign for the subtraction sign. But probably this is a slip either in the algebra or in the printing. (See corrigendum below—Ed.)

CORRIGENDUM

In 'The normal distribution of intelligence: a critique' by D. G. Lewis, Vol. **48**, Part , the formula on p. 99 should read as follows:

$$E_{13}=\frac{S_1-S_3}{\frac{1}{3}n}.$$

ON THE VALIDITY OF APPLYING COMMUNICATION THEORY TO EXPERIMENTAL PSYCHOLOGY*

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Communication theory is properly applied to human performance only under certain conditions: for example, the application must be made by an outside person and not by the subject himself. The theory also requires averages of numerous events rather than dealing with isolated ones. Given these and similar conditions, some useful and valid applications may be made to psychology. It is perfectly possible to apply the theory in the case in which stimulus and response are continuous functions of time, although this is not always realized.

I. Introduction

We shall discuss briefly some of the difficulties of using the formal Mathematical Theory of Communication (in the sense of Shannon, 1949) as an aid to constructing and interpreting experiments involving human behaviour. Note that the title refers to '...validity of *applying*...'; it is not the validity of the mathematical theory itself which is here in question, but only the suitability and correctness of its application to certain types of experiment. My remarks will be illustrated with one or two well-known psychological experiments, as models. It will be assumed that the reader is already familiar with the principal methods and theorems of Communication Theory.

We can observe a human operator or two humans forming a communication link (as for example, a conversation) only by setting up another channel—the *meta-channel*, or *channel of observation* (Fig. 1).

The Shannon Theory of Communication is written in the language of this observer. It is built upon the concepts of a message source ensemble, with message probabilities $p(x)$ and a noisy† received signal ensemble, with probabilities $p(y)$. It uses also conditional probabilities, such as $p_y(x)$ and others. Now these probabilities can be assessed only by an observer who can observe both the transmitted signs x and the received noisy signals y. That is, by an observer *who can see both ends of the channel*—an external observer, as in Fig. 1. Expressions such as x, y, $p(x)$, $p(y)$, $p_y(x)$, etc., are then proper only to the meta-language of such an external observer and not to that of the receiver who knows only the received signals y. The recipient of the signals can observe only those signals y and estimate their probabilities of arrival; he cannot observe the sent message x except inasmuch as he can regard y as evidence about them. But he cannot directly observe x, nor can he estimate probabilities $p(x)$.

In physical, including psychological, experiments, observations are made and reported upon; and the nature of the reports, the theory set up, will depend upon whether the observer is completely outside the phenomenon he observes, or whether he himself is part of it. For example, in astronomy the observer, by looking through his telescope

* A revised version of a paper read at a Symposium on Human Operator Problems, organized by the Ministry of Supply, 19 September 1955, London.

† By 'noisy signals', we mean signals which are subject to disturbances which are not known in moment-by-moment detail, but only statistically, e.g., random disturbances.

oes not affect the stars in their courses. By contrast, in many laboratory experiments e observer may disturb the observed phenomenon by his very observations; or he may ed to make changes, or alter some situation, before the phenomenon comes to light. ut experiments upon 'communication' are particularly prone to disturbance by the server as when for instance, this observer is one of the communicants. By an external server then we mean one who merely watches as through a peephole and records; he akes no disturbances once his experimental situation is set up. We shall later discuss e consequences of confusing these two kinds of observer. In psychological experiments e are dealing with a unique class of phenomena—human beings, who are themselves articute and can make their own reports. Both languages have their uses; that of the exrnal observer and that of the human subject under observation. But, as stressed ready, Shannon's Communication Theory is written wholly in the meta-language of an ternal observer; he himself exhibits this fact by figure 8 of his book (1949), and by e theorems which he derives from the assumptions shown in that figure.

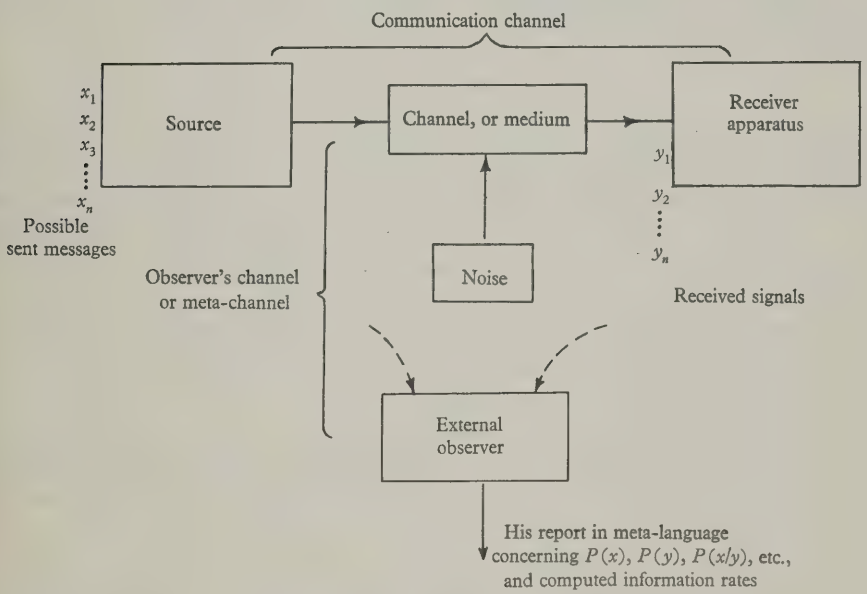

Fig. 1. A communication channel and its external observer.

II. The basic assumptions of Shannon's Communication Theory

shall start with a bare statement of the basic assumptions of Shannon's mathematical eory and then proceed to examine them, in relation to the design of human operator periments. The theory was first set up to describe the properties of a communication urce, channel and receiver, as exampled, say, by a teleprinter channel; such a system not precisely analogous to a human operator with his sense-stimuli and overt reponses. It will therefore be helpful to distinguish between (*a*) a communication channel d (*b*) a human operator. Let us list the assumptions of the theory as relating, most operly, to the former.

(A) The theory is expressed entirely in the (meta-) language of an external observe not in that of one of the participants (Fig. 1). We observe a communication channel onl by setting up another—the 'Observation Channel'.

(B) Shannon's theory is a statistical one; that is, it concerns only average, macro scopic, properties of a system, not specific momentary properties.

(C) From long series of past observations it is assumed that the external observe has made good estimates of the probabilities (frequencies) of occurrence of the variou transmitted message signals x, received signals y and interfering noise. Further, it assumed that the noise source(s) can be identified ('singled out') and their statistica properties specified by the observer; or, in certain simple cases, that noise is absent.

(D) It is assumed that, *a priori*, the two communicants operate with an agreed set message signs, or alphabet, having certain definite frequencies of occurrence. It assumed that these frequencies remain unchanged with the passage of time; that is, tha the statistics of the situation are stationary.

(E) It is assumed that this alphabet of signs $x_1, x_2, \ldots, x_n$ (signals, ensemble of mes sages, coding system) remains unchanged. (We beg the question of so-called 'continuous information sources for the moment.)

III. The human operator ('subject') compared to a communication channel

Comparison between a human operator and a communication channel is to-day commor place; the stimuli represent 'input signals' applied to his sense-organs, while his ove responses represent 'output signals'; the human operator represents the 'transducer But there are a few essential points to be borne in mind, when using this analogy:

(*a*) A human operator is an integrated organism, and his ears, eyes, nose, finger-tip etc. are not like independent input terminals; the responses made depend upon the co ditions at all the sense organs, which are mutually dependent. What a man thinks h can smell depends upon what he sees, and so on.

(*b*) A human being is an ensemble of experiences; as a 'transducer', his parameter or properties, may change after every communicative experience (i.e. perception Again, he cannot be represented by one set of fixed parameters, because these may de pend upon the nature of the stimulus itself and of the environment.

(c) Every individual represents a different ensemble of experiences, so that inpu stimuli become associated with very varied sets of prior stimuli in determining responses that is, the 'meaningfulness' of stimuli is a fluid and varied property of human informa tion-transfer, rendering it difficult to make the choice of stimuli sufficiently universa in behavioural experiments. We may try to smooth out the variations by choosin groups of people of similar educational background, in a close age-group, etc. Again fo stimuli we try to choose letters, words, signs of various kinds, such as may be assume to be 'widely known'. But Communication Theory, in Shannon's terms, makes no re ference to 'meaningfulness' but only to *syntactic* properties and rules—only to signs an their probabilities, joint probabilities, etc., as assessed by the external observer. Therefore

(*d*) If we choose to ignore the 'meaningfulness' of stimuli, but rather to regard stimul like letters, words, etc., purely syntactically as some philosophic schools will have (t which, incidentally, I do not belong) then we really are assuming that the responses of human operator to these stimuli will depend only upon the range of choice they offer an

heir probabilities of occurrence. In the experiments of Hick (1952, 1953), for example, his assumption is reasonably justified, since his stimuli are provided visually by a imple ring of lamps, the 'meaningfulness' of which will surely be fairly universal and of ittle influence. In his examples, Communication Theory gives a good, valid interpretaion. But, furthermore, Miller (1951) cites several examples of stimulus-response experiments using letters and words, but in which the results are interpretable, objectively and epeatedly, in terms only of probabilities, without reference to 'meaningfulness'.

It seems to me to be one of the major discoveries of recent behavioural experiments, that esponses to linguistic stimuli are interpretable this way; certainly some credit for this s attributable to the discipline of statistical Communication Theory. But more on this ater.

(*e*) We have mentioned 'probabilities of stimuli'. Probabilities to whom? If we take ny one specific person (human operator) we cannot speak of the 'probability' of a certain etter or word to him, as a frequency; doubtless, such probabilities are represented, natrix-wise, in the structure and organization of his brain—but they are not assessable, ither externally, or by the operator himself. Probabilities, to him are 'degrees of belief' nd at best may be rank-ordered, but are non-numerical. But the Theory of Communicaion takes probabilities as relative frequencies; hence:

(*f*) If we use linguistic stimuli, we may use the relative frequencies of the letters and vords 'in the language' as the probabilities, and then average our results over large umbers of human operators drawn from this language group. In contrast to Communicaion Theory, which usually refers to an ensemble of messages, we often require also an nsemble of operators (subjects).

(*g*) The difficulty of 'non-stationariness', corresponding to learning ability, may be ealt with in the same way. In a communication channel, we may assume the apparatus as fixed, definable, properties like bandwidth, noise-level, modulation limits, etc., but oes not change its structure with time and 'experience'. Communication Theory then ommonly treats *time*-average statistics of input and output signals. But human operators earn, and conditions of test may not necessarily remain stationary with time; however, ve may nevertheless take *ensemble*-averages, on the assumption that an ensemble of perators will change, or learn, in nearly-like manner, under the same test conditions.

(*h*) One most important aspect of the human operator in comparison to a communicaion channel, which arises when Communication Theory methods are used, is easily overooked. The human operator, the organism, cannot be 'taken apart'. Even if regarded heoretically, as a 'black box' transducer having an unspecified arrangement internally, t it questionable whether we can validly subdivide its apparent functions and discuss hem separately, as we commonly do in the case of the various parts of an electrical ommunication channel.

To illustrate these important points (*a*)–(*h*) it is convenient to return to our distinction etween the external observer and the participant observer, referring to Figs. 1–3.

External observer and participant observer

The external observer (Fig. 1), as assumed by Shannon's Communication Theory, can bserve both ends of the communication channel. He observes the transmitter's alphabet f message signals $x_1, x_2, \ldots, x_n$ and assesses their relative frequencies $p(x_1), \ldots, p(x_n)$; ie observes the noise 'signals' and the received signals $y_1, y_2, \ldots, y_n$ similarly. If we

imagine the channel as being, say, a long length of telegraph cable, and the 'receiver apparatus' as a teleprinter, it is sometimes convenient to treat these separately. For example, the received signals y may be observed as noisy signals received at the end of the cable, say at the terminals of the detector in the receiving apparatus; the observer may then compute the transmitted information rate $H(x)$ and the equivocation $H(x/y$

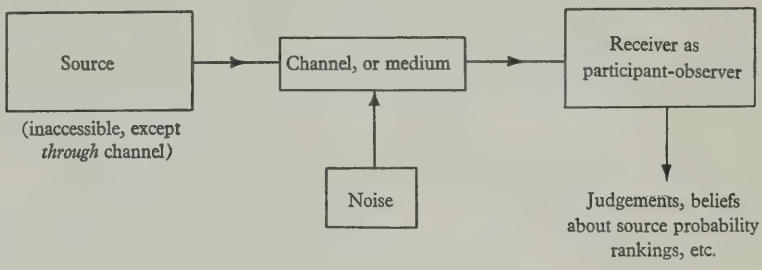

Fig. 2. The participant observer, or receiver as his own observer.

(average ambiguity about x existing potentially in received signals y), thereby giving the rate of transmission of information R between the source and the receiver detector terminals. Here:

$$H(x) = -\epsilon(\log p(x)) = -\sum_x p(x) \log p(x),$$

$$H(x/y) = -\epsilon(\log p(x/y)) = -\sum_x \sum_y p(x, y) \log p(x/y),$$

$$R = H(x) - H(x/y) = \sum_x \sum_y p(x, y) \log \frac{p(x, y)}{p(x)\, p(y)}.$$

This last expression may of course be written $\epsilon\left((\log \frac{p(x/y)}{p(x)}\right)$ so that we have the valid concept of the 'rate of communication of information', between these two points, as the expected value of the logarithmic ratio of two probability distributions, the source distribution $p(x)$ and the conditional probability $p(x/y)$. Again, as is well known, this rate of information may be written another way:

$$R = H(y) - H(y/x) = \epsilon\left(\log \frac{p(y/x)}{p(y)}\right),$$

giving the concept of true information rate as the difference between two rates, that potentially existing in the received noisy signals, $H(y)$, some being bogus, and the rate of this bogus information, $H(y/x)$, arising from the noise. These rates are assessable only by the external observer for only he sees both ends of the channel and hence 'knows' x.

The point to be emphasized here is that this ratio R is described, in this way, entirely in terms of complete probability-*distributions* (averaged, at that); information is expressed by the conversion of a prior to a posterior *distribution*. No reference is made to the final process required in any physical, useful, receiving apparatus, such as a teleprinter—namely, the printing of some unique key-sign each time a signal is received (its 'overt behaviour', or reactions). The best the detector can do is compute the required probability (or likelihood) distribution; some threshold mechanism must deter

mine the specific choices of x out of $x_1, x_2, \ldots, x_n$ at each signal-receipt, as communication proceeds. This choice then throws away information each time it is made.

Nevertheless, in a communication system, the observer can equally well observe the printed letters of the teleprinter. Some will fail to correspond with the letters at the source, and errors will occur; the observer can take these printed letters as being the 'received signals' y and so, regarding now the teleprinter apparatus as being part of the complete channel, compute the rate of information right through, from the source keyboard to the receiver keyboard. Thus the same measure can be applied at successive points along a communication system, all being accessible, and the rates of information calculated, up to each of these points, as required.

Now a word about the participant observer. By this name we would imply, when discussing communication systems, one who is situated at the receiver end, and having no access to the source except *through* the communication channel itself. He can assess the probabilities of the transmitted message-signals x only by inference from his noisy received signals; again he may have no access to the noise source, to observe it. Consequently, he cannot describe the whole communication process; communication theory, very properly, is not expressed in *his* language. He cannot, for example, compute the information rate R, nor the channel capacity C, because both involve x.

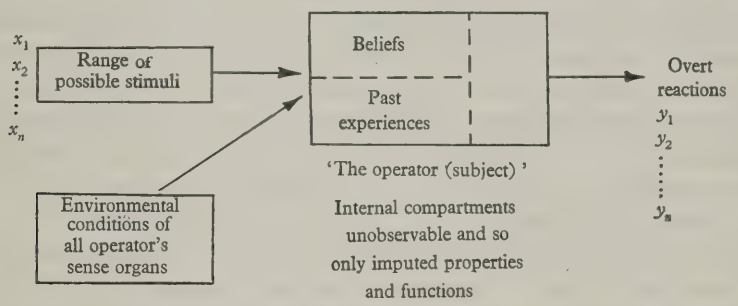

Fig. 3. The human operator (subject) and the transfer of information, in behavioural experiments.

Information transfer and decision making

In communication theory we discuss the transfer of information, and express it quantitatively, in terms of the ratio of a prior to a posterior probability distribution. Thus, if x represents a set of possible sent message-signals $(x_1, x_2, \ldots x_n)$ which become perturbed by noise, and if y_e is a unique noisy signal on some *one* occasion, then the receiver can be regarded as computing the likelihood function $L(x)$ where $L(x) = p(y_e/x)$. The *whole* of this function represents received information. This does not determine which particular x of the set $(x_1, x_2, \ldots, x_n)$ should be taken as the truth, and this *decision* depends upon some criterion of detection built into the receiver. The whole of this function contributes towards information about x; if one x be chosen as 'true', and the other possibilities rejected, then information is lost. Communication theory does not, as it stands at present, consider the bases on which such decisions may be made, though the whole theory is concerned with this very type of problem.

Let us consider a typical psychological experiment, in which an experimenter selects signals from a set of alternatives and applies them as stimuli, one at a time, to a human

operator (subject). The environment is perhaps noisy, or disturbing in other ways, so that the operator is in the situation of having to identify the alternative signals selected, from these perturbed stimuli, perhaps whilst he is under stress. For example, the signals selected may be random words, the stimuli spoken utterance of them, while the environment may contain acoustic noise; the operator may then be required to guess the words from the sounds he hears.

It is convenient to think of the brain of the human operator as being a superb device designed to form and weigh hypotheses according to evidence supplied to it. Thus, with reference to Fig. 3, a stimulus received in noise or other environmental uncertainties constitutes evidence to the operator about the possible selected signals (alternative hypotheses); the operator may use this evidence and weigh his various alternative hypotheses (the likelihood function), finally deciding and making overt reaction, representing his final decision (not necessarily consciously). But from his overt behaviour alone we cannot discuss this decision process, at least in the language of Communication Theory, for this requires an external observer. Decisions are made in ways which depend not only upon the evidence and the operator's past experience, but perhaps upon his expectations of the future consequences of the decisions too. Decisions are taken in varied ways, and may depend upon maximizing chances of survival, or bodily comfort, or one's economic situation; in fact upon a range of possible 'utilities'.

An external observer (who is entitled, at least, to try and apply Communication Theory to his observations) can observe only the applied stimuli, the environmental conditions and the operator's overt responses. He cannot observe, or report upon, the subject's hypotheses, his likelihood estimations or his decision processes, at least separately. He can at best speculate. But it is possible to design psychological experiments in which constraints operate so as to define clearly a set of alternative hypotheses (as, for example, in Hick's experiments, 1952, 1953).

Although the complete likelihood function represented in the subject's brain cannot directly be observed, it may be taken into account descriptively, in a familiar way. Thus, before a stimulus arrives, the subject will have formed a number of hypotheses, each relatively weighted, concerning this stimulus (prior 'likelihood') representing his 'prior state of expectancy'. The receipt of this stimulus, in the particular disturbing environment, will normally change this weighting distribution, resulting in the subject's changed attitude, set, or 'posterior state of expectancy'. Depending upon whether the successive stimuli are related or not, the subject's 'state of expectancy' towards subsequent stimuli may be changed. Such 'states of expectancy' are, descriptively, like likelihood functions and might be correlated with actual functions (matrices) represented neurally in the brain. But in psychological experiment these are not normally observed. The subject's overt reaction to a stimulus is observable, and this is *assumed* to depend upon the 'prior state of expectancy' and the stimulus; but the precise nature of this dependency ('decision process') cannot readily be observed.

Nevertheless, it may be possible for an external observer to *infer* something about the likelihood function in the subject's brain (giving 'states of expectancy') by observing long series of overt reactions to successively applied stimuli.

IV. Some examples of experimental results validly interpreted in Communication Theory terms

should like now to give a few illustrations from recent well-known experimental work, ut I must emphasize that any criticism offered does not reflect upon the value of the xperiments *per se*, or on their method; my comments refer solely to the use of Com-unication Theory and concepts in their interpretations.

(i) *The choice reaction-time experiments of Hick*

In these experiments, visual stimuli are derived from a number of small lamps, ranged in a circle, lit one at a time at 5 sec. intervals in virtually random order and with qual frequencies. The subject responds by touching one of a corresponding number of eys. It is found that the total time taken to respond is not a constant 'reaction-time', ut increases as the logarithm of the number of lamps or keys in the set (with a certain orrection at the low number end). These experiments are now familiar so that de-cription may be left in such brief, fragmentary form.

In such experiments Hick showed a full appreciation of the assumptions of Communica-on Theory. Thus, it is reasonable to assume that one lamp is much like another, and so emantically neutral. In his earlier experiments, Hick operated his successive lamp-ghting at low speeds, so that zero errors occurred; there was then no question of 'noise', of 'equivocation'. Further, he used long sequences and averaged the results; he also ook the precaution, which we noted earlier, of averaging the results over an ensemble f operators. The results were observed by an independent 'external observer'. So all the ecessary conditions applied.

In a second series of experiments, Hick required his subjects to shorten their reaction mes, even at the expense of making errors. This situation corresponds to a noisy ommunication channel if, as before, x represents the transmitted signals (stimuli), and the received ('noisy') signals (responses). Then R, the rate of information transfer is btained from the formulae given above:

$$R = H(x) - H(x/y),$$

here $H(x)$ and $H(x/y)$ can be assessed from the frequencies $p(x)$, $p(x/y)$ of stimuli and esponses over the set allowed.

Notice that here R is not the rate of information about x to the subject (operator). is the rate of transference *through* him, as though he was a noisy or unreliable part of a ommunication link. This is a perfectly valid and useful concept, and it does not involve ne 'likelihood estimations' and 'decision process'.

Again, no attempt has been made to give exact interpretation of the operator's 'capa-ty' in the technical Communication Theory sense of that word; for 'capacity' involves ne concept of the hypothetical maximum information rate, over all possible forms of oding of the stimuli (Shannon, 1949). And we cannot try all possible forms. However nese experiments suggest that R might be used as a valuable measure of the degree of natching between an information source and a human operator.

(ii) *Application to intelligence tests*

By contrast, I do not find myself in such ready sympathy with Hick (1951) in his uses f Communication Theory concepts with reference to intelligence tests. In his paper, he

first discusses deduction and deduction-induction as the possible processes which ma be applied to a test question, in order to frame an answer, with the inevitable probabilistic nature of any answer on the latter basis. I would agree with the author about th hollowness of arguments raised against induction by some theorists; induction is all w have (besides which, equally weighty arguments can be applied against the concept 'deduction', e.g., see J. S. Mill's *Logic*). Hick then draws analogy to the extractio of messages from a noisy communication channel; the 'questions' used in intelligenc tests represent 'noisy signals' and the 'answers' represent 'extracted messages'. Th operator, whose intelligence is being tested, is then 'a predictive filter'.

Functionally speaking, this comparison is interesting, but it is the next steps wit which I incline to disagree. 'Noise' is compared to the 'agency of the psychologist, wh frames the questions', and 'is probabilistic *from the subject's* (*operator's*) *point of view* (italics mine). But in Communication Theory the receiver has not got a point of view it is written wholly in the language of the external observer. Further, Hick discusse the probabilities of rules as applying to questions as they are *to the subject*. Again, suc probabilities, as beliefs, are non-numerical and do not come into the Theory of Commun cation. We may certainly discuss 'information' qualitatively, in such terms, but no numerically.

To repeat, I am not questioning the validity of Hick's results or theory; I simpl wish to stress that it goes beyond established Communication Theory as this is used f numerical *measurement*.

(iii) *The construction of statistical approximations to language*

In my opinion, one of the most valuable additions to method in experimental psy chology has come, from Communication Theory, with the suggestion of 'probabilit texts'.

In a human language, such as English, a great degree of redundancy arises from th existence of syntactic rules, which are obeyed by its users; such rules are variously d scribed as rules of spelling, rules of grammar, etc. Of course, the user himself may n know the rules, any more than he knows *why* he laughs at a joke. Rather it is the studen of language, the linguist and grammarian, who describe the rules, as they discover ther the user merely conforms to *habits*.

Rather than describe the rules in the sophisticated way of modern grammarian Communication Theory gives means of assessing the constraints upon printed language terms of sets of conditional probabilities. From counting great seas of printed texts w can assess frequencies of monograms, di-grams...*n*-grams, letter-bridges, etc., and d scribe the rules statistically. Unfortunately, it is questionable whether similar statisti could ever be found and prescribed usefully for spoken language. For speech is not spoke writing; it is unpremeditated and freely breaks all the rules; it is fragmentary and u grammatical; and all conversations are specialized and topical.

Statistics of many printed languages have been assessed, but are reliable only up tri-grams at the very most. Using these tables, sample tests may be constructed whi approximate to the language. For closer, *n*-gram, approximations a working techniq may be employed which is due to Shannon (1951), and which may be used for constructi both letter sequences and word sequences: a sequence of *n* words, chosen from sor existing text, is shown to a member of the language group, who is asked to guess t

ext word; then the first word of this $(n+1)$ sequence is covered up and the new sequence f n shown to another member, who in turn guesses a suitable next word...and so on. 'ery large numbers of people from the same culture are required for constructing such probability texts' of any length to be representative of the language, on the assumption hat they conform to their engrained language-habits, on an average.

Such 'probability texts' read as nonsense, but the degree of nonsense is controlled and s assessable *quantitatively*. The use of letters, words (nonsense or real), and sentences or ong texts, as stimuli in psychological experiments is of course very old. But in the past has largely been left to the psychologist to 'invent' or 'design' such texts to what he onsidered various standards of sense, meaningfulness, familiarity,..., according as his xperience had taught him. But using this probability technique we can construct texts vhich are, say 10% or 50% like English, on an average. Their uses for experiments are ndless (Miller, 1951).

To refer back to Sect. III*d*; 'meaning' is essentially a *relationship* between a stimulus and s recipient. We are not denying that 'meaning' and 'meaningfulness' are not perfectly ood, useful, concepts. Rather, it seems that the use of 'probability texts' by-passes hilosophical discussion and substitutes a quantitative measure to the stimulus-recipient elationship, averaged over a population of recipients, and expressible in the meta-anguage of an external observer. For instance, reaction times may be found to vary ccording to the 'meaningfulness' of word or sentence stimuli; but, alternatively, nsemble-average reaction times may be related to the probabilities of these stimuli, uantitatively.

Tachistoscopic experiments

It has been known, at least since 1885, that speed of visual perception, as exampled y reading, increases with familiarity with the language. Using these 'probability texts', he exact functional relationship can be established. Employing tachistoscopic presenta-ion Miller, Bruner and Postman (see Miller, 1951, for various applications to experimental sychology, including this experiment) measured the degree of correct identification of -letter 'words' having respectively random order, and first-, second- and fourth-order ransition-probability approximation to English, when the visual exposure times were aried. Table 1 illustrates some of their test-'words'; from left to right these becoming rogressively and measurably closer to English, on an average.

Table 1

Random order (zero redundancy)	1-gram (15% redundancy)	2-gram (29% redundancy)	4-gram (44% redundancy)
YRULPZOC	STANUGOP	WALLYOFF	RICANING
OZHGPMTJ	VTYEHULO	THERARES	VERNALIT
DLEGQMNW	EINOAASE	CHEVADNE	MOSSIANT
GFUJXZAQ	IYDEWAKN	NERMBLIM	BITTLERS
WXPAUJOS	RPITCQET	ONESTEVA	ONETICUL
etc.	etc.	etc.	etc.

Miller, Bruner and Postman have shown that although the 4-gram 'words' are recog-ized more quickly and accurately, in tachistoscopic tests, than those of poorer approxi-nation to English, the rate of information intake (or rather *transfer*) is roughly constant. The 4-gram 'words' are recognized more quickly, but they are also more 'redundant'.

Provided that the results of such tachistoscopic tests are averaged over long series o stimulus words and, again, averaged over many subjects who are within the languag group, such uses of the Wiener-Shannon information measure are perfectly valid, an very useful. Qualitative results become quantitative.

This illustration provides a good example of non-stationary conditions. Thus w cannot validly take time-averages because, if the tachistoscopic exposure is long enough *every* letter of a word will be recognized without error. But averaging may be don instead over an ensemble of people (ensemble average).

'Probability texts' may be used for various quantitative psychological experiment upon visual perception and discrimination. For verbal association tests, for memory test (recall, or learning), and for articulation tests under noisy conditions, for example. The writer has suggested elsewhere their use for assessment of the perceptual rates of visua information whilst reading—the relation between the frequency and distribution of ey saccades and the probability of the textual matter (Cherry, 1955).

These varied examples of psychological experiments which I have cited have bee very few in number, but may serve to illustrate the points which should be borne in min when interpreting results quantitatively with the statistical 'information-rate' measure

V. Information rates are relative to a given source ensemble

The ring of N lamps used by Hick (Sect. IV (i)) and the 'probability texts' just describe represent two ways of excluding 'meaningfulness' from experiments and thereby causing serious personal variations of response. In Hick's case, one lamp is as good as another semantically almost neutral; in the 'probability texts', each dummy word may certainly be meaningful to any one subject, in the sense that it will set up images and associations in his mind, but the trouble is presumed to be overcome by the smoothing which take place when results are averaged over many subject's responses.

These two kinds of stimuli represent different types of source ensemble. Hick's lamps form an alphabet of N members, having equal or other assigned frequencies; the words constructed for 'probability texts' may also be used in alphabets of N words, but their probabilities will, subjectively speaking and only on an average, conform to those o letters in 'the English language' represented by the habits of the subjects who are drawn from an English population. (Strictly, we should not speak of '*the* English language' we might, for some purposes, distinguish journalese, scientific English, nineteenth-century novels, and a host of other languages. But a great number of statistical counts have been published, e.g., see Zipf, 1949.)

It is then important to appreciate that information-rates are always determined relative to some given source ensemble; an alphabet, with its set of probabilities. Information rates are not absolute, but relative to some frame of reference, in any specific practical case. When information rates are quoted, the ensemble should be specified, that is, in relation to psychological experiments, the exact stimulus conditions must be noted.

I have mentioned particularly visual and aural stimuli but, with this precaution in mind there is no reason why reliable and quantitative rates should not be found for other sense-stimuli, taste or smell for instance (Beebe-Center, Rogers & O'Connell, 1955). One can always set up an empirical alphabet of smells or tastes and measure information rates *relative* to that frame of reference.

VI. The unreality of 'continuous information'

ny tracking experiments may appear at first to use a 'continuous' stimulus function; example, when sighting a gun on to a moving target, or when following a wandering e on a moving band of paper with the finger. Such notions raise the concept of 'con-uous information'—a concept which to my mind has no place in physical experiments and I should like to end by commenting upon it. But first, I would remark that 'con-uous information' may well be a useful *mathematical* concept, for mathematicians ire) deal with constructs, not with experiments and observations.

As is well known the information rate of a hypothetical noiseless source $H(x)$ can be inite; 'continuous information' can be discussed meaningfully only if noise is considered, that the equivocation $H(x/y)$ is infinite to the same degree. Their difference $R = H(x) - x/y)$ is however finite, so that everyone is satisfied. But in tracking experiments it ay be difficult and undesirable to specify or account for any noise. The signal to be acked is 'continuous' in a 'noise-free' environment. Then the question arises: is the rate information supplied to the operator infinite?

This question is not answered by observing that the operator's perceptions and reac-ns must be discrete and finite in number. For instance, it might be pointed out that e operator's eyes do not track continuously, but in discrete saccades, his perceptions king place during the fixation pauses only, when his eyes are temporarily at rest. deed, it would seem to me that 'perceptions' are of such a nature that they *can* only cur at points in time. The decision is made, the working hypothesis selected, at some tant; perceptions cannot be 'continuous', by their very nature.

Again, all human sense-organs have a minimum threshold, below which they are not tivated; finite changes in stimulus are needed to set up perceptions. However, these jections do not answer our question, because they refer not to the source of information, t to the recipient's perceptions and reactions.

If the question raised here be considered proper to Communication Theory, the answer it is *no*. For it is the external observer who gives the answer, not the operator himself, d when the observer describes anything he can say only a finite number of things out it. If the 'continuous' curve being tracked consists of a wavy line on paper, the server can state the position of a finite number of sample points to a finite accuracy. it he cannot describe the whole curve. It may be objected that he could quote the uation of the curve; but still he is giving only an equation, symbolically, not a curve awn on paper. Again, he cannot specify the tracking subject's responses, his moving ger positions for example, with mathematical precision.

In practice, the information rate of a physical 'continuous' source, as may be used in cking experiments, could be specified by choosing some agreed set of spaced samples l dealing in discrete terms, with the amplitudes of these samples quantized. The muli and responses are reduced to a finite set, with a finite number of degrees of freedom. e information rate of the source, and also of the operator's reactions (output rate) *specifiable only with respect to some prior agreed discrete set of choices.*

But the concept of a 'continuous information' source certainly has its uses, and per-ps a distinction may be drawn which clarifies the point.

Pure and applied Communication Theory

Like other branches of mathematics, Communication Theory has its 'pure' side, which the concept of a 'continuous' source rightly belongs. Continuity is essentially mathematical concept and, of course, a most valuable simplifying concept. It has be easier, quicker and more fruitful to the development of mathematics to deal with co tinuous functions than with great masses of finite data.

Again, there are those whose prime interest is in Communication Theory as mathematical discipline, and not in its application to the description of physical co munication situations. Whereas the physical situations call for an observer, in who meta-language the description is made, mathematics calls for no observer at all—n meta-channel of description.

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ON SOME PSYCHOPHYSICAL TECHNIQUES EMPLOYING A SINGLE MANIPULANDUM

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Four techniques are described which permit the single bar-press apparatus to be used for the generation of psychophysical functions. Rate of response is a useful dependent variable for measuring the strength of behaviour associated with the negative stimulus in two of the methods. When a warning signal is introduced with discrete negative and positive stimuli, the situation becomes logically akin to the traditional infra-human psychophysical procedures. Rate of response is then no longer useful as a variable, but because any given response does not alter the animal's geographical position, the bar-pressing situation offers certain advantages over the traditional procedures. These advantages are incorporated in the two remaining techniques described.

I. Introduction

recent years there has been an increase in the use of automatically controlled 'vending achine' types of apparatus, in which manipulation of a single lever, bar, or panel can made to deliver food, or other reward, according to conditions set for the experiment. or an excellent description and analysis of the single manipulandum situation, see rster, 1953.) Most commonly, these devices have been employed for the study of haviour under conditions where there are no environmental stimuli which are distinc- e occasions for rewarded responses—such as the study of rates of response under riable or fixed intervals schedules of reward (Skinner, 1953). Occasionally, they have en used for the investigation or demonstration of discriminated behaviour (Skinner, 53; Heise, 1953). The latter application is likely to increase in the future, if for no other son than that bar-pressing units are approaching the status of standard equipment many laboratories, thereby tempting research students by their availability, and be- use the units have been made automatic to such an extent that considerable benefits offered in economy of time and flexible control of stimulus scheduling. It is worth quiring, therefore, into the relationship between the bar-press, or single manipulandum uation, and procedures traditionally employed for the study of psychophysical problems. ch an inquiry should yield information of both practical and methodological value.

For practical purposes, the single manipulandum situation is limited to successive criminations, although simultaneous discriminations are not beyond the range of ssibility. Its traditional counterparts for infra-human subjects, therefore, are such uations as the Y-maze, the single-stimulus Lashley jumping stand, and the Wisconsin crimination apparatus when arranged for a 'go—no-go' or approach-avoidance crimination. The bar-press situation, however, can differ in two respects from these ier situations. In the first place, each unrewarded response leaves the animal in actly the same physical situation as before the response, and hence rate of response be used as a dependent variable. In the second place, in the traditional situations stimulus permits the animal to choose one of two alternative responses, i.e. *where* to

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respond, while in the bar-press situation, the stimulus may only tell the animal *whe* to respond. But, as we shall see, the bar-press situation also can be made logicall equivalent to traditional situations.

The limitations of a given procedure are more evident when one considers psycho physical functions rather than simple discriminations. There is little difficulty in estab lishing a 'discrimination' between, say, two widely separated acoustic tones, if one ton is always made the occasion for a given schedule of reward to be operative, while th other is never the occasion for a reward. Indeed, this serves commonly as a class-roo demonstration. But it would be impossible to determine the *DL* (i.e. least discriminabl difference) between two tones with these contingencies, at least not without imposin rather strict limitations on such factors as stimulus duration relative to average rewar interval. Consider the instance of a stimulus difference which is subliminal, or in th limiting case, zero. Given even a low 'operant' level of response rate, the animal woul be rewarded occasionally for responding to a stimulus difference which was undetectable when this occurred, it would be to the animal's advantage to continue responding. Th discrimination could be based on the occurrence either of a reward *or* of a supralimina stimulus difference. Put in other terms, the difficulty lies in the non-independence rewarded responses over a period of time; given one rewarded response, the probabilit is high that a response during a succeeding given interval will also be rewarded, quit independently of the presence or absence of any other discriminative stimuli. One ste towards avoiding this difficulty is to make the reward terminate the positive stimulu All of the methods discussed here will be subjected to this condition. But it is evide from what has been said that psychophysical requirements refine the issues to be raise in evaluating a discriminative situation. All of the illustrative material here, therefor will be of psychophysical determinations.

All successive discriminations share one difficulty. In the early stages of discriminatio training, there is usually a high degree of 'generalization' or 'induction' between the tw stimuli; i.e. the animal makes the same response to both stimuli. In a 'go—no-g discrimination, for example, where the animal is required to make a response to o stimulus (the 'positive' stimulus) and to refrain from making that response to the oth stimulus (the 'negative' stimulus), the animal will sometimes continue to respond both stimuli. Since indiscriminate responding produces intermittent reward, and sin intermittent reward schedules are known to produce relatively high and stable rates response, the pattern may be an exceedingly persistent one. The real problem, then, oft lies in first reducing the probability of an incorrect response to a negative stimulus rath than increasing the probability of a correct response to a positive stimulus. One of th traditional methods of achieving this is to punish incorrect responses. The use of aversiv stimulation, however, involves many additional difficulties, such as the suppression of a behaviour in the situation. Fortunately, other procedures are available which do n employ aversive stimulation. Some of these will be discussed below. They can be place in one or both of two classes: (1) extinguishing the incorrect response to the negativ stimulus—the negative stimulus is presented either much more frequently or for long duration than the positive stimulus; (2) making the appearance of the positive stimul contingent on the subject's not having made an incorrect response to a negative stimulu Both classes of procedures have several variants, each of which has special problems but these are problems which are basically inherent in the actual meaning of discrimin

on. In this sense, it is a truism that there is no available technique which is actually, potentially, devoid of such problems, any more than there is a technique which is not a nction of variables other than the stimulus dimension.

This report will discuss four specific psychophysical procedures which have been olved for use in the bar-pressing situation. The first two find no parallel among the aditional procedures, in that they employ rate of response as a dependent variable. he last two have traditional counterparts, but the bar-press situation offers certain lditional advantages. All illustrative material will be drawn from two types of threshold udies: the difference limen for frequency, using acoustic stimuli; and the critical fusion equency, using optic stimuli. Subjects include pigeons, rhesus monkeys and humans.

II. Continuously changing stimulus method

the positive stimulus is terminated by reward, it can be asked whether rate of response s any value as a dependent variable—a single response to the positive stimulus can eld as much information to the experimenter (and to the subject) as N responses over a ne interval. But there is still the negative stimulus, and here rate of response might be ed effectively. One ingenious possibility has been reported by Skinner (1953), who udies the behaviour of pigeons with respect to what he termed an 'external clock'. straight line on the front panel of the cage was made to increase in length at a constant te, the bird being rewarded for responding only when the line had reached a certain aximal length. After the reinforcement, the stimulus reverted to its original value d went through another cycle. The characteristic behaviour which finally emerged was lack of bar-pressing until just before the stimulus had reached its final value, when e bird responded at a high and relatively fixed rate until rewarded. The *DL* can be fined as the value at which the animal begins to respond, the mid-point between this lue and the final reward value, or some other fixed value.

Two stimulus parameters can be assigned. The first is the stimulus difference between e starting value and the final value; the second is the rate at which the change of the mulus occurs—or, in more general terms, the form of the transition function from arting to final value. These factors determine, of course, the temporal length of the cycle om start to finish. As yet, no completely systematic investigations have been made of these rameters, but there is evidence which suggests that variations in these factors can pro- ce large differences in behaviour, and that the relationship is probably not a simple one. Skinner, after comparing cycle periods varying from 3 to 32 min., states that 'the te of responding is roughly the same for a given size of spot regardless of speed of change. ie curve at 32 min. is obviously not approximately 10 times as high as that at 3 min., it should be if the control by the spot were strictly equivalent' (p. 74). This means that e *DL* became smaller as the temporal period increased. In the published curves (which e unfortunately too small to permit precise measurement), however, there is a suggestion a rather smoother and more gradual start with long periods than with short periods, d the starting-points, in terms of stimulus value, might be roughly the same.

Using shorter periods of 1 or 2 min., the behaviour can be shown to be affected by the mulus difference between the starting and final values. A tone was made to warble the rate of three times per second) between a value of 2600 c.p.s. and another variable equency. The second frequency was gradually and continuously lowered until the tone

remained steady at 2600 c.p.s.; the pigeon was rewarded for responding to the steady tone. The entire run took 56 sec., the steady tone remaining on for the last 5 sec of the run. After the steady tone, there was a 5 sec. period of silence, and a new run was begun. The initial warble value could be varied without changing the decay function. In fig. 1 are shown three such values, plotted against rate of response. Each point is actually an average of 15 runs. It can be seen that as the starting value is lowered the animal begins to respond earlier in the run. But the point at which the animal re sponds is not solely under the control of the absolute stimulus value. If mid-points are chosen as the *DL*'s, the values are 55, 30, and 11 c.p.s., respectively. Substantially the same results were obtained with a run of 2 min., although the data are not as complete for that value.

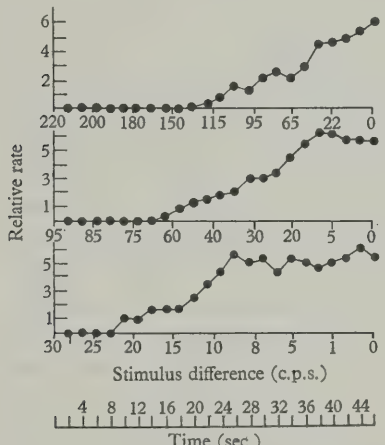

Fig. 1

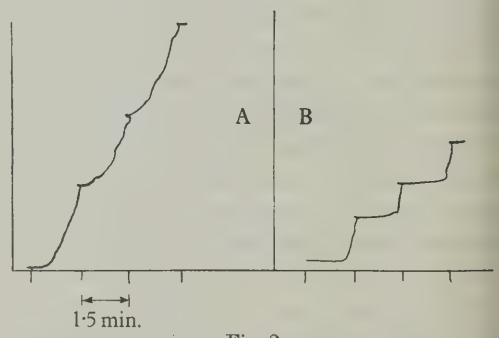

Fig. 2

Fig. 1. Average relative rate of response for a pigeon as warbling tone approaches steady tone. Three curves ar for three different initial warble ranges.

Fig. 2. Successive cumulative response curves for a pigeon as warbling tone approaches steady tone. (A) Rat of stimulus change is independent of the animal's response. (B) After 2½ hr. training in which 'earl starts' locked the stimulus until animal stopped responding.

It is quite likely that if longer runs were chosen, the behaviour would tend to becom less and less a function of the initial stimulus difference, but, at least within the rang explored, the initial value can influence the relationship considerably. It will also be note that the curves in Fig. 1 show a fairly gradual increase in rate as the stimulus approache the steady tone. This was not produced merely by virtue of the averaging of values. Th response pattern, typically, showed a 'scallop' rather than a 'staircase', effect. Fig. 2 shows a tracing of three successive runs made by the pigeon immediately after he ha yielded the data for Fig. 1. Again, such scalloping might tend to sharpen up with longe runs or with increased training—the present curves were the product of 14 days of training 2 hr. daily.

It is possible, however, to sharpen up the curves by means of one of the genera methods discussed in the Introduction. When human subjects were trained on thi discrimination (see below), a common verbalization was 'the tone was still warbling bu I thought I might as well start early and make sure'. There was no contingency, in othe words, forcing a low rate of response during the negative stimulus. In order to eliminat

rly starts, an arbitrary stimulus can be chosen such that, if the animal begins to respond a larger value, the stimulus is instantly stopped and kept at that value until after sponding has stopped. Early starting thereby delays the appearance of the steady tone, d therefore of reinforcement. That this technique was effective in sharpening up the haviour is clear from the cumulative tracings shown in Fig. 2. In this experiment a n of $1\frac{2}{3}$ min. was used, including a 10 sec. silence period. Fig. 2A represents the re-onding at the beginning of the experimental session, without the added 'stimulus cking' contingency. Fig. 2B shows the same animal $2\frac{1}{2}$ hr. later, after the contingency d been introduced, but in these particular trials it had been removed, and conditions ere otherwise the same as in Fig. 2A.

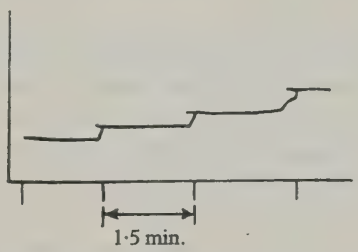

Fig. 3. Successive cumulative response curves for a human subject as warbling tone approaches steady tone, using the 'stimulus lock' device described in text.

Human subjects can be trained to make a similar discrimination. The subject is pro-ded with a handswitch and a set of earphones. His only instruction is that he is to make ight on a panel flash on as often as possible. Roughly the same training procedure is en employed, including the addition of the 'locking' of the stimulus for early responding. own in Fig. 3 is a cumulative response curve, using a warbling tone which approaches 00 c.p.s. This subject started to press at about 8 c.p.s. difference from the steady tone. aining time has been found to vary among subjects from as little as 30 min. to as long 6 hr.

In general, it is felt that this method has not yet reached a point of refinement where it ovides a useful measure of discriminative capacity. Under certain conditions, it is tremely sensitive to general parametric conditions and, in addition, is probably very pendent upon length and method of training adopted. These are not 'faults' of the thods; all methods are affected similarly, but, in the opinion of the author, this par-ular technique is more sensitive to these factors than are many other procedures. ere is an additional difficulty in general method—this will receive discussion in a later tion. But, if for no other reason than the speed with which psychophysical functions n be generated, the method deserves further investigation.

III. BACKGROUND EXTINCTION METHOD

the previous method, the stimulus 'locking' device was of the second type described he Introduction for reducing the frequency of incorrect responses to negative stimuli—king the appearance of the positive stimulus contingent on the subject's not having de a bar-press to the negative stimulus. The other technique—extinction—is well

represented in the present method. As before, rate of response is used as a dependen variable with respect to the negative stimulus, but an additional dependent variable i introduced in the simple percentage of responses to the positive stimulus. The psycho physical function is calculated as a joint function of both variables.

The negative stimulus is presented continuously as a background against which shor durations of the positive stimulus are presented on an aperiodic schedule. For example in the determination of the absolute threshold for acoustic intensity, long periods o silence are presented between short presentations of tones of varying intensity. Heis has used such a technique quite successfully for determinations of the minimum audibl field of the pigeon (Heise, 1953). In another example, in the determination of critica fusion frequency, long periods of steady light are presented between short presentation of flickering lights of varying frequency.

There will always be some responding in the presence of the background or negativ stimulus. Such responding, which Heise has termed 'noise' if it is of significantly hig rate, must enter into the calculation of the threshold value, since it can provide a hig 'chance' probability of a response to the positive stimulus. A simple mathematical cor rection for noise is available (Heise, 1953). The most effective policy, of course, is to lowe the noise level to a negligible value, rather than to correct for a high level. One metho is to increase the mean interval of the aperiodic schedule. For a pigeon under ordinar conditions of deprivation, a schedule with a mean of 2 min. has been found quite satis factory for the auditory discrimination just described. An additional possibility, on used by Heise, is to increase the number of responses required for reinforcement durin the appearance of the positive stimulus. For example, one can require five bar-presse in 5 sec. This, in effect, redefines the unit response.

Regardless of these controls, noise level tends to increase to a considerable value whe the positive stimulus is maintained at just supra-threshold values. For this reason, i is important to ensure that very long sequences of difficult values are not presented Using this technique, therefore, to obtain thresholds by the 'quantal method' (Stevens Morgan & Volkman, 1941) for infra-human organisms is not always feasible, although i seems at first glance to be almost ideal. More practical procedures are to present value either randomly or in short sequences of a given value.

Another factor to be taken into consideration is the prolonged presence of the negativ stimulus. If the problem is one in which fatigue or adaptation is likely to be a factor this method may have serious drawbacks. In a later section, inferential evidence will b presented suggesting that such an effect can be significant under certain conditions i auditory discrimination.

Fig. 4 shows a psychophysical function for acoustic frequency for a pigeon, obtaine with the Background Extinction Method. The negative background stimulus was steady tone of 2600 c.p.s., 60 db. sensation level (human), while the positive stimulu was a frequency-modulated tone which warbled at the rate of three times a secon between 2600 c.p.s. and another variable higher frequency; as the variable frequenc approached 2600 c.p.s., it became more difficult to distinguish a warble from a stead tone. The variable frequency is plotted on the abscissa; the ordinate represents th percentage of stimulus presentations to which there was a response of at least 10 ba presses. The positive stimuli were presented on a 2 min. variable interval schedule, eac stimulus having a duration of 5 sec. The positive stimulus value was kept consta

for four or five presentations, and then changed to a new value randomly. Each point represents at least eight presentations. The *DL* (50% point) for this bird was approximately 25 c.p.s. No correction was made for noise, since it was found to be negligible.

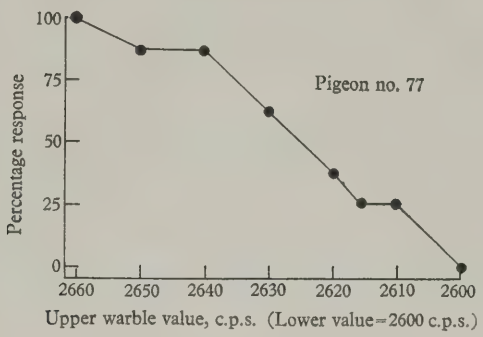

Fig. 4. Pigeon's psychometric function for acoustical frequency, obtained with the Background Extinction Method.

IV. Method of limits

Each of the previous methods has at least one difficulty associated with it, methodological in one case, technical in the other, which has not yet received attention. The Continuously Changing Stimulus Method evidently is insensitive without a stimulus-locking device, as described, since it is never to the animal's disadvantage to begin its response run early. But the locking device introduces a delicate problem of the actual definition of the *DL*.

The Background Extinction Method suffers from no such vagueness about the definition of the threshold but, when used for discriminations made along certain stimulus dimensions, it presents a serious technical problem. For example, consider the case of the *DL* for acoustic frequency. In changing from the background of frequency A to the positive stimulus of frequency *B*, acoustic transients will be produced—varying as a function of the actual transition curve. These transients may be audible as a click. A related difficulty may be created by the switching mechanism; there may be a brief period of non-stimulation or of summation of both stimuli. With carefully designed apparatus, these problems can be eliminated—but often only after considerable investment of money or time. In general, the Background Extinction Method, for these reasons, is more suitable for absolute rather than for relative thresholds.

An alternative approach is one in which the artifact is deliberately retained. On some occasions, however, the negative stimulus is substituted for the positive, a device sometimes called the 'introduction of blanks'. It is of interest to note that this simple alteration provides the bridge between the bar-press situation as it has been used heretofore and the traditional situations. In the latter, the animal is always given a cue or warning signal for the *opportunity* for response—it may be the raising of a screen or the placing of the animal in the starting box. Concomitantly, this alteration renders rate of response superfluous as a variable. Instead, responses to positive stimuli are compared with responses to blanks.

The following method is one which attempts to meet both of the methodological and technical difficulties just discussed. It bears its closest relationship to the method of limits as used in human psychophysics. The method of limits, without modification, is

clearly unsuitable for animal work, since the reward contingency cannot be establishe satisfactorily—one cannot reward a 'correct' response. To illustrate the method use here, an example will be given of the determination of the *DL* for acoustic frequency employing a warbling stimulus as described above.

Discrete tones of 5 sec. duration were presented. The first tone presented always had warble range of fairly high value (i.e. greater than 100 c.p.s.). If the bird did *not* respon to the tone, the next tone was a lower value of the warble range. Similarly, if it did no respond to this, the next tone was a still lower warble. The animal was rewarded if responded when the tone finally was steady, as it *might* have been after 4 or 5 steps. the bird *did* respond to a warbling tone, regardless of its range value, the duration of th tone was extended for an additional 5 sec., during which time it was changed quit quickly to a much higher value of warble range. Then on successive trials the animal woul be forced to start over again from this higher value, working down to lower values b not responding. In other words, any 'mistake' would necessitate starting the series d descending steps over again, thereby delaying the possibility for reward.

On the basis of how well the animal did in the earlier stages of training, a 'critical area of warble range was selected. It was in this area that the animal no longer consistentl discriminated the warbling tone from a steady tone (i.e. he sometimes responded to th warble). The critical area included two warble values, an upper and a lower. From table of random numbers, two schedules were prepared, one for the upper and one for th lower value. If the animal reached the upper value in the critical area, a steady tone wa presented 25 % of the time instead of the usual warble range value. If he then responded he was rewarded. If he did not respond to the steady tone, the series was begun agai A similar procedure was adopted for the lower value, except that on 75 % of the occasion a steady tone was substituted for the warble. Hence, in effect, the schedule was one whic tended to improve the discrimination, since the lower value was more heavily weighte With such improvement, the animal would refrain more frequently from making in correct responses to the lower warble value, in which case a steady tone was presente as the next stimulus. If the animal correctly refrained from responding to more tha half of the lower warble presentations (out of an arbitrary number of presentations) th entire critical area was lowered. Similarly, if the animal was incorrect on more than ha of the upper warble presentations, the critical area was raised. The *DL* with this procedur is defined in terms of the point at which the animal mistakenly responds to a negativ stimulus.

The threshold values which have been determined in this fashion possess a high degr of stability. It is usually possible to run a pigeon for at least 2 hr., obtaining approx mately 100 successive threshold values without any apparent large shifts in values. O principal advantage of the method lies in the speed with which successive *DL*'s may obtained. For the data here discussed, a value was obtained, on the average, once in period just longer than a minute. For this reason it lends itself to the study of short-ter changes in thresholds. For example, Deutsch (1951) has reported a temporary decrea in the human monaural *DL* for pitch following stimulation of the contralateral ear f 4 min. by a tone of the frequency from which the *DL* is being measured. This effect termed an 'auditory figural after-effect'. A similar phenomenon (with binaural stimul tion) in the pigeon is suggested by Fig. 5, which shows successive *DL*'s for pitch 2600 c.p.s. for a pigeon before and after a 4 min. presentation of a 2600 c.p.s. to

When the 4 min. tone was presented, the overhead light was turned off, which effectively topped the pigeon's responding.) In Fig. 6 is shown similar data for another pigeon, ogether with control data for the effect of a 4 min. 'dead time' period. In both graphs, here is a suggestion of a lowered *DL* following the 4 min. tone, with a gradual reversal back to its normal level after some minutes. (The *DL*'s shown in Figs. 4 and 5 were obtained from the same pigeon, no. 77. It is interesting to note that the *DL* following satiation' in Fig. 5 is roughly the same as that shown in Fig. 4. The latter was obtained by the Background Extinction Method, in which there was a steady tone present continuously, except for short bursts of the positive warbling stimuli. It may be argued, herefore, that both figures show the effects of a satiation tone on the *DL*.) Repetition of the Deutsch experiment using human subjects and the present technique, however, uggests that the effect is found only under rather special conditions, and the present data are certainly not complete enough to be taken as positive confirmation of his findings. They are presented, rather, to illustrate the use to which the method may be put.

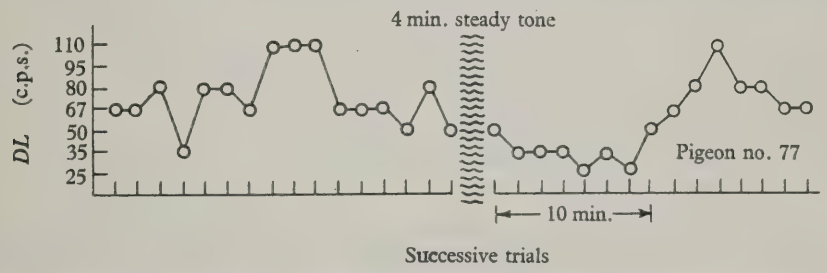

Fig. 5. Successive *DL*'s for pitch from a tone of 2600 c.p.s., obtained with the Method of Limits, before and after a 4 min. satiation tone of 2600 c.p.s.

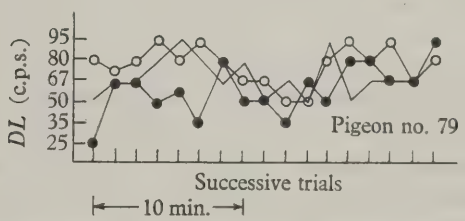

Fig. 6. Successive *DL*'s for pitch from a tone of 2600 c.p.s., before 4 min. blackout period, after 4 min. blackout and silence period, and after 4 min. blackout and satiation tone. ○—○, Before blackout; ———, after blackout and 4 min. silence; ●—●, after blackout and 4 min. steady tone.

V. SYMMETRICALLY-REWARDED 'GO—NO-GO'

So long as both stimuli are presented discretely, with some sort of accompanying warning signal, a great many stimulus schedules are possible. The present method is perhaps the most general, in that both stimuli are presented randomly. It is the analogue of the human 'method of constants'. This method has the advantage over the Method of Limits of permitting almost complete automaticity of procedure, and, concomitantly, in removing the experimenter as a controlling and possibly biasing factor. It loses, however, the advantage of speed in threshold determination.

The basic contingencies are simple. A stimulus is presented every 10 sec., say, with a maximal duration of 5 sec. If the stimulus is positive, the animal is rewarded for a bar-press.

If the stimulus is negative, the animal is rewarded at the end of the 5 sec. period for *not* responding. If the animal behaves incorrectly with respect to either stimulus (fails to press to the positive or presses to the negative), the stimulus is repeated until he behaves correctly. Rewarding correct 'non-responses' to the negative stimulus ensures that every sequence of initial-plus-correction presentations ends with rewarded behaviour, whether the stimulus is positive or negative, and tends to distribute errors to both stimuli more evenly than the usual non-reward method. Such a negative stimulus reward contingency would be difficult to establish in the conventional 'go—no-go' situation. In the Y-maze, it does exist, but only by virtue of the animal's having to alter its physical location by proceeding into a goal-box. The bar-press situation, in addition to permitting negative stimulus reward, can present the sequence of stimuli at a rate not usually possible in conventional procedures.

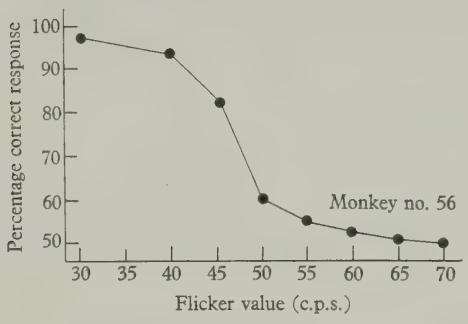

Fig. 7. Psychometric function for a monkey in a discrimination between flickering and steady light. Data obtained with 'symmetrically-rewarded go—no-go method'.

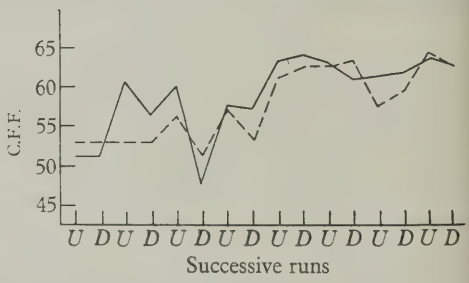

Fig. 8. Successive values of cf.f. for two monkeys. *U*, refers to ascending series; *D*, to descending series. ——, Monkey no. 153; – – – –, monkey no. 156.

In order to obtain a threshold determination, the value of the positive stimulus is varied either randomly or in a systematic order. Technically, it is simpler to keep the positive stimulus at a fixed value for a single experimental session, varying its value systematically from session to session. A variety of scoring conventions is available; one simple method, the one applied to the data below, is to score the percentage of correct responses to the non-correction (i.e. initial) stimulus presentations. Since at worst, the animal is correct 50% of the time, on the average, the threshold is defined as the 75% correct response point, i.e. the point at which it is making the discrimination half of the time.

This method was developed (in an unpublished study by Mishkin and the author) to determine the critical fusion frequency for normal and brain-operated rhesus monkeys. In fig. 7 is shown a plot of the percentage of correct responses to various values of a flickering light, projected on a diffuse lucite disk just under the bar. Each point represents eighty trials in which the flickering light was randomly alternated with the steady light. The flickering light was generated by a projection lantern and an episcotister with an on/off ratio of 1·00. The 'steady' light was generated by a parallel episcotister constantly presenting a flickering light of 140 c.p.s., with a matched equal brightness of 5·25 foot-candles. The screen diameter was 0·75 in.

While, for this example, the flicker was successively increased until the animal's correct responses approached 50%, in practice it was thought more advantageous not

o force the discrimination to 'chance' too frequently, not only for the sake of efficiency, ut also to avoid the conditions for classical Pavlovian neurosis. The procedure adopted vas to raise the flicker value until the animal received a score of less than 75%, then to ower the value until it received a score of better than 90%. In this way one could come ip and down the scale many times without difficulty. In Fig. 8 is shown a plot of the 5% point on successive up and down runs for two rhesus monkeys. Each point is the nterpolated value in a series going from above 90% to below 75%, as just described.

The experimental work reported here was conducted at the Psychological Laboratories, Harvard University, and at the Department of Neurophysiology, Institute of Living. Part of the research was supported by Contract DA-49-007-MD-401, Department of the Army, Washington, D.C. The author would like to express his appreciation to Drs Mortimer Mishkin, Karl H. Pribram, and William A. Wilson, Jr., for their helpful suggestions in the preparation of this paper. Mrs Marilyn Tucker and Miss Lila Rupp greatly assisted with technical and mechanical aspects of preparing the manuscript.

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PERCEPTUAL AND PERSONALITY DYNAMICS IN REACTIONS TO AMBIGUITY

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A battery of tests designed to elicit differential responses to a variety of ambiguous situations was administered to groups of Control and Neurotic subjects. The data show marked individual differences and considerable evidence for intra-individual consistency in avoidance/non-avoidance of ambiguous situations. A high percentage of significant correlations suggests that the tests measure few, closely related variables. Age and intelligence are only minimally involved. The evidence is taken to be consistent with a theory of cognitive control of the environment mediated via perceptual attitudes operative at varying levels of the individual.

To test the influence of controlling and restrictive personality factors on reactions towards ambiguous situations, groups of anxious, hysterical and obsessional psychiatric patients were compared for tendencies to avoid ambiguity. Neurotic subjects were generally shown to avoid ambiguity more than Controls, and Conversion Hysterics and Obsessionals avoided ambiguous situations more than Anxiety States. It is suggested that avoidance of ambiguity serves to avoid anxiety and conflict. The similarity in behaviour between Obsessionals and Conversion Hysterics is interpreted as being due to the functional similarity and aims of the neurotic symptoms of these two clinical groups.

I. Introduction

'Intolerance of ambiguity' was first conceptualised by Frenkel-Brunswik (1949), who defined it as 'preference for familiarity, symmetry, definiteness and regularity,...a tendency toward black-white solutions, over-simplified dichotomizing, premature, unqualified either/or solutions', etc. These tendencies she considered to be perceptual characteristics found in association with strong attitudes of social and ethnic prejudice, and with repression as a dominant defence mechanism. The high-prejudice group of Frenkel-Brunswik's investigation was also characterized by an inability to think in terms of probability, and by an avoidance of uncertainty 'as accomplished by the narrowing of meaning and the mechanical repetition of set' (1954). This last aspect of the definition would appear to reduce 'intolerance of ambiguity' to a variant of rigidity in the sense in which the latter is used by Luchins (1942) and Rokeach (1948). Eysenck (1954), however, reports over-all low correlations between measures of 'intolerance of ambiguity' and perceptual and conceptual rigidity, so that any obvious similarities between these two variants may be only a function of definition and experimental technique.

'Intolerance of ambiguity' has been variously investigated. Block and Block (1950–51) find the phenomenon expressed in speed of norm establishment in an experiment with the autokinetic phenomenon. Kaplan (1952) reported a contingency between 'intolerance of instability' in the perceptual field induced by means of aniseikonic lenses, and avoidance of equivocality and contradictory material. Further demonstrations of 'intolerance of ambiguity' and its relationship to flexibility of thought (Blum, 1954), need for clarification in ambiguous inter-personal situations (Martin, 1954), and specific

* This investigation forms part of a thesis for the degree of Doctor of Philosophy in Psychology deposited with the University of London Library. It was written while the writer held a Research Studentship at Birkbeck College, University of London, 1953–55.

ntecedent conditions of psychological stress (Smock, 1955*a*, *b*), suggest that the indivi-ıal's response to ambiguous perceptual situations may depend on a specific organization personality to deal consistently with perceptual situations lacking structure, defini-on and certainty.

Experimental evidence in support of a principle of generalized cognitive control has een advanced by Klein and his collaborators (1951*a*, *b*, *c*, 1953*a*, *b*, 1954). According this theory, cognitive control of the environment is achieved through the operation of erceptual attitudes' of levelling, form-boundedness, intolerance of instability and so on.

The aim of the first part of the investigation reported here was twofold. First, to etermine if there are consistent individual differences in response to a variety of mbiguous situations. Secondly, to determine whether consistency of response to ambi-ıity is sufficiently marked to serve as support for the view that consistent perceptual ttitudes are instruments of an 'adaptive principle of ego control'.

These aims were formulated in the following questions: (1) Are there individual dif-rences in avoidance of ambiguity as measured by a battery of tests? (2) Are subjects nsistent in their responses to ambiguity from test to test? (3) Are differences in avoid-ıce of ambiguity related to differences in age, intelligence, sex or occupation?

(i) *Description of tests*

Eleven test situations were chosen as satisfying the criteria of an ambiguous situation. nly ten of them will be discussed here. The eleventh, apparent movement, will be treated sewhere in greater detail. Empirical evidence had been obtained in a pilot study ıggesting that the tests discriminated sufficiently well between individuals and that gnificant correlations between them were consistent with the measurement of only a w related traits.

Test 1. *Weight Discrimination.* Sixteen uniform black cans were compared with a can of standard eight. Twelve of the cans were objectively equal in weight to the Standard, two were noticeably lighter d two noticeably heavier. Subjects were told that none of the variables was equal to the Standard, d were required to sort all the weights into the following three categories: 'Heavier', 'Lighter', and Iay be Heavier or Lighter, I can't decide which'.

Test 2. *Length Discrimination.* Sixteen lines on uniform background were compared with a line of andard length. Twelve of the lines were objectively equal to the Standard, two were clearly shorter, d a further two equally clearly longer. Subjects were told that none of the variables were equal to the andard, and they were required to sort the lines into the following three categories: 'Longer', 'Shorter', Iay be Longer or Shorter, I can't decide which'.

Test 3. *Brightness Discrimination.* Subjects were required to compare sixteen light stimuli with a ht of standard brightness. Twelve of the stimuli were of objectively equal brightness to the Standard, o were clearly brighter, and two were clearly dimmer. Subjects were told that none of the stimuli ere equal to the Standard, and they were asked to assign their judgements to one of the following three tegories: 'Brighter', 'Dimmer', 'May be Brighter or Dimmer, I can't decide which'.

Test 4. *Block Sorting Test.* This consisted of forty-four wooden blocks of varied dimensions and shape. urteen were rectangular or triangular, five were circular, and twenty-five incorporated both rectangular d circular features in the same block. The sorting categories in this case were 'Circular', 'Rectangular', Iay be Circular or Rectangular, I can't decide which', and a fourth category, 'To be sorted into separate oups afterwards'.

Test 5. *Ambiguous Drawings I. Sheaf–tree series.* In this, the first of three series of ambiguous drawings, me of the 12 drawings were clearly sheaves, some were clearly trees, while others were indefinite or ntained in the same drawing, simultaneously, features characteristic of both trees and sheaves. The rting categories were: 'Sheaf', 'Tree', and 'May be either a Sheaf or a Tree, I can't decide which'.

Test 6. *Ambiguous drawings II. House–car series.* This was similar to test 5, except that the two objects in the drawings were a house and a car. The sorting categories were: 'House', 'Car', and 'May be either a House or a Car, I can't decide which'.

Test. 7. *Ambiguous Drawings III. Car–dog series.* This series was different from tests 5 and 6 because in this case, none of the drawings incorporated features of both objects at the same time. The intention in most of the drawings here was to present rather vague or indefinite designs for classification. The categories were: 'Dog', 'Car', and 'May be either a Car or a Dog, I can't decide which'.

Each series was presented in random order, with all the drawings of each series being laid out in front of the subject at the same time. This procedure was adopted to counteract any possibility of an 'Einstellung' effect. On completion of a series, subjects were asked to re-sort the drawings which they had placed into the middle category into an 'Either' group and a group which consisted of drawings which were considered to be really 'Neither' object 1 nor object 2.

Test 8. '*Ambiguity*' *scale.* This scale set out to measure those aspects of an ambiguous situation which will be defined as lack of absoluteness, indefiniteness, and equivocality of interpretation, with special reference to social situations. There were thirty-three items, ten of them 'dummies', which had to be 'posted' into boxes marked 'True' and 'False'. The following are two examples of the type of statement on which judgement had to be made. 'A solution to a personal problem cannot be both right and wrong—it is either one or the other.' 'I hate leaving a decision to hang in the balance—I feel I simply have to decide either one way or the other.'

Test 9. *Reversible Perspectives I. Rubin figure.* Oscillations of the 'Double Cross' figure were measured by an electric counter operated by the subject, over two periods of 30 sec each. The oscillations were measured under two experimental conditions: (1) autonomous reversals, subject passive; and (2) controlled reversal with subject attempting to reverse as often as possible. Under both conditions, subjects were required to fixate the centre point of the design and to control blinking.

Test 10. *Reversible Perspectives II. Necker cube.* Conditions and design were identical with test 9. The calculated reliability of reversals under autonomous conditions for the two reversible figures was 0·844 and 0·876 respectively.

These tests were administered to 102 subjects. Data concerning the constitution of the experimental groups are found in Table 3.

(ii) *Results of correlational study*

Correlation coefficients were calculated using Spearman's Rho formula, corrected for ties by the formula given by Kendall (1948). This was considered to be the most economical method of handling the data, and since no assumptions about the normality of the distributions were made, it may be considered valid. Garrett (1947) and Kendall (1948) have demonstrated that, with a sizeable sample, Rho does not differ materially from Pearson-Bravais r.

Table 1. *Means, standard deviations and range of tests*

	Discrimination tests, C.D. scores			Blocks		Ambiguous drawings, 'Either' scores			Ambiguity scale	Autonomous Reversals		Controlled Reversals	
Test	Weight	Length	B'ness	C.D.	'Others'	Sheaf-tree	House-car	Car-dog		Rubin	Cube	Rubin	Cube
Mean	2·76	3·95	3·84	5·58	19·68	1·13	0·98	1·01	9·77	9·89	9·95	19·32	17·29
S.D.	2·23	2·99	2·72	6·31	11·47	1·38	1·37	1·32	3·43	8·21	5·12	11·93	7·21
Range	0–11	0–12	0–12	0–27	1–43	0–7	0–5	0–5	1–17	0–49	1–26	0–65	0–56

C.D. = 'Can't Decide' scores. 'Others' = blocks which are neither rectangular, nor circular, nor C.D.

Table 1 shows that the data were rarely normally distributed. The results obtained from the Discrimination Tests indicate that individual differences in 'Can't Decide' (C.D.) responses are quite considerable. It appears that in no case did the number

D. responses exceed 12. This suggests that the four objectively unequal stimuli in each st were in fact perceived as being different. Table 2 indicates that a considerable ercentage of inter-test correlations is significant at the 1% or 5% level, and that more an 75% of them are above ±0·100. From the matrix a fairly clear picture is obtained how the tests are grouped together. Two groups may thus be distinguished: the first nsisting of the Discrimination Tests, the Block Sorting Test and the series of Ambigu- us Drawings; the other of the tests of Reversible Perspective and the 'Ambiguity' Scale. he two groups are interrelated by virtue of the correlations between the Ambiguous rawings and the Ambiguity Scale, and the correlations between the Discrimination ests and the tests of Reversible Perspective.

In order to exclude the possibility that the relatively consistent significant correlations etween the tests could be accounted for in terms of consistently high correlations for ne or two of the experimental subgroups only, separate correlation matrices for each of ne experimental subgroups were obtained for seven of the most highly correlated tests. mparisons of correlation coefficients, obtained from each of the experimental groups parately, indicate that none of the 105 correlations is significantly different from any her. On the other hand, the expected over-all decrease in the size of the correlation efficients with the increase in homogeneity of the smaller samples materialized in only % of the new correlations. This finding suggested that the heterogeneity of the sub- mples with respect to the measured behaviour of avoidance of ambiguity may be nsiderable, and that, on some of the tests at least, the differences between some of the perimental subgroups might well not be significant.

(iii) *Discussion*

Some form of relationship between the variable tapped by the tests, and intelligence nd age was bound to be demonstrated, since there are few forms of behaviour, especially the cognitive sphere, which cannot be shown to vary with both intelligence and age. fact, it might be thought that intelligence is maximally involved in any process quiring perceptual judgements. In the present study, intelligence correlated with the Ambiguity' Scale and the Rubin figure under both autonomous and controlled con- tions of oscillation. It might therefore be held that the correlation demonstrated etween the Scale and Reversible Perspective is spurious. Partial correlation invali- ates this argument ($r12{\cdot}3 = -0{\cdot}284$ and $-0{\cdot}293$) and shows that the tests are still gnificantly related when intelligence is held constant.

The position for age is similar. Evidence has accumulated (Welford, 1951) to suggest at the older section of the population would tend to have the greater difficulty in deal- g with new experiences, and that this deterioration is particularly evident in lack of ility to organize new stimuli and select the relevant ones for differential response. For ese reasons age might have had considerable influence in the present investigation. rrelations, however, were shown only with the Scale and with number of oscillations the Rubin figure under controlled conditions. Partial correlation does not materially tract from the original correlation between these two experimental tests, and the new rrelation is still significant at better than the 1% level.

The individual differences in avoidance of ambiguity, the consistency of the behaviour functionally related situations as demonstrated here, are taken to support Klein's

Table 2. *Correlation matrix*

Significance level of r: $\frac{N-2}{100}$ 5% 0·195 1% 0·254

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Weights, C.D.	1	.	0·425	0·385	0·201	−0·130	0·381	0·325	0·357	0·158	0·204	0·231	0·201	0·204	0·057	0·020
Length, C.D.	2	.	.	0·538	0·219	−0·041	0·353	0·408	·0374	−0·061	0·312	0·122	0·265	0·123	0·130	0·083
Brightness, C.D.	3	.	.	.	0·273	−0·195	0·195	0·216	0·278	0	0·195	0·070	0·221	0·170	0·120	−0·103
Blocks, C.D.	4	.	.	.	.	.	0·296	0·195	0·184	−0·050	0·010	−0·040	0·020	0·010	−0·003	0·030
Blocks, 'Others'	5	.	.	.	.	.	−0·293	−0·256	−0·253	−0·030	0·040	0·130	0·030	0·090	0·020	0·030
Sheaf–tree	6	.	.	.	.	.	.	0·661	0·585	0·285	0·202	0·175	0·120	0·135	0·060	−0·032
House–Car	7	.	.	.	.	.	.	.	0·591	0·304	0·195	0·150	0·146	0·067	0·060	−0·056
Car–Dog	8	.	.	.	.	.	.	.	.	0·232	0·095	0·083	0·044	0·118	−0·020	0·021
'A' Scale*	9	.	.	.	.	.	.	.	.	.	0·340	0·286	0·360	0·350	0·262	−0·219
Rubin, Autonomous	10	.	.	.	.	.	.	.	.	.	.	0·539	0·740	0·470	0·300	−0·094
Cube, Autonomous	11	.	.	.	.	.	.	.	.	.	.	.	0·310	0·550	0·072	−0·103
Rubin, Controlled	12	.	.	.	.	.	.	.	.	.	.	.	.	0·560	0·369	−0·266
Cube, Controlled	13	.	.	.	.	.	.	.	.	.	.	.	.	.	0·141	−0·118
Intelligence	14	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
Age	15	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.

(* The signs for this test have been reversed).

1951*a*, 1954) theory of perceptual attitudes and systems of cognitive control. The degree f consistency from situation to situation is strongly in favour of a relatively permanent nd pervasive organization corresponding to some kind of need state or motivational set' of the individual.

An explanation of the data in terms of differences in avoidance of ambiguity is preerred to a number of *ad hoc* explanations, mainly because it is comprehensive. The data ay be taken to show that several of the dichotomies previously isolated by Klein, such s form-boundedness and intolerance of instability and overlap, tend to be correlated. is posited that, functionally, the consistent perceptual attitude operative in all the tuations had, as its object, the control of ambiguity and uncertainty—the control of definiteness, unstructuredness and instability to a level required by a central principle f personality organization or motivational need. This explanation is consistent with a eory (Beier, 1951; Cowan, 1952; Smock, 1955*a*, *b*) that specified perceptual behaviour ay be predictably changed by modifying the anxiety level of subjects. More light could us be thrown on the problem at the explanatory level by investigating the responses to nbiguity of individuals differing in a relatively well-known manner in their affective ehaviour, or personality structure. This has been attempted in the second part of the vestigation.

II. A COMPARISON OF PSYCHIATRIC AND CONTROL GROUPS

(i) *Theoretical considerations*

he original study on 'intolerance of ambiguity' by Frenkel-Brunswik (1949) suggested at differences in this behaviour may be specifically related to the individual's methods f dealing with conflicts and anxiety in the environment. This suggestion finds support the study by Rosenberg (1953) on the preference for symmetry of Obsessive-Compulsive eurotics, and the work of Barron & Welch (1952), who reported a significant correlation etween preference for symmetry and Hysteria score on the M.M.P.I. It is further supported by the findings of Balken & Masserman (1940) to the effect that Conversion ysterics and Obsessive-Compulsive Neurotics show a need for certainty, definition, ualifying and classifying in their responses to objectively scored TAT cards. A pilot udy briefly reported elsewhere (Hamilton, 1955) indicated that the tests used in e present study were able to differentiate between Control and Neurotic subjects, and so between the Neurotic subgroups themselves. The results led to the search of the inical literature for experimental and non-experimental evidence consistent with these ndings.

Freud (1920, 1926) postulated that there is no neurosis without anxiety and conflict, hile Abrahams (1942) considers neurosis a 'flight' from or a compromise to a conflict f. also Symonds, 1946; Maslow & Mittelmann, 1941). Thus the Neurotic may be expected attempt to avoid conflicts or anxiety-arousing situations. The Conversion Hysteric is nsidered to display this avoidance tendency prominently in his ability to repress and his facility for producing somatic conversion symptoms (S. Freud, 1893, 1920, 1926, c.; Brown, 1940; Hadfield, 1950). The Conversion Hysteric's disinclination to face fficulties, his dependence an his ability to dissociate would all appear to be operative the service of conflict avoidance, an avoidance which may be considered to have its ots in methods first adopted in infancy to deal with undesirable or unacceptable wishes

or tendencies. According to Freud (1926), repression may be found to precede the mechan ism of reaction formation in the psychogenesis of Obsessionality, a process in which a unacceptable idea is said to become isolated from its affect. Fenichel (1945) deals a length with the similarity between this affective type of isolation and the Obsessional' system-building and classificatory tendencies. Such a mechanism would predispose th Obsessional to classify ideas, events, and objects into mutually exclusive categories it would lead to relations being considered as being of the 'either...or...' rather tha the 'as well as' kind. By forcing the world into his own system, the Obsessional, therefore retreats from reality just as the Conversion Hysteric does (Fenichel, 1945). In th Anxiety Neurotic these mechanisms are few or non-existent so that he finds it difficult t avoid anxiety and conflict. Where no alternative is available, the Anxiety Neurotic ma respond to additional stress by 'leaving the field'. Suggestive evidence from comparativ psychology supports the view that conflict consists of incompatible response tendencies and that neurotic conflict or 'experimental neurosis' may be considered to follow upo 'mutually exclusive, opposite tendencies of relatively absolutely equal strength' (Bitter man, 1944; Miller, 1944; Sears & Hovland, 1941; Russell, 1950; *et al.*). For the purpose o this study, an ambiguous situation was defined as having double or dubious meaning, a being vague, unstructured or subject to fluctuations, as being of doubtful classification and engendering subjective uncertainty. The tests of the battery were constructed t meet these criteria and to give rise to mutually exclusive response tendencies. On th basis of the above considerations, it was predicted that Neurotics and Controls, and th Neurotic subgroups themselves, might respond to ambiguity in a way that was congruen with their customary manner of handling difficulties, conflicts and situations of doubtfu designation in their environment. The test battery consisted of the tests described i detail in the Introduction.

(ii) *Formulation of problem and method*

It was felt that present-day formulations of psychoanalytic theory were too general t permit the formulation and testing of specific hypotheses, and that a scientifically rigorou procedure was probably best served by preceding it by the accumulation of furthe empirical data. To this end, the problem was phrased in the form of specific questions to which the empirical data were hoped to supply fairly unequivocal answers. First what differences are there, if any, in avoidance of ambiguity between a Neurotic and Control group? Secondly, what differences are there, if any, in avoidance of ambiguit between the Neurotic subgroups? Thirdly, could any group differences in avoidance o ambiguity be accounted for by differences in age and intelligence?

Table 3. *The experimental groups*

	Controls	Anxiety states	Obsessionals	Conversion hysterics	Neurotics
Men	18	10	12	11	33
Women	22	12	8	9	29
Mean age	30·7	33·4	33·7	32·9	33·3
Range	17–68	21–50	21–54	22–47	21–54
Mean I.Q.	110·3	112·9	110·6	109·6	111·0
Range	100–130	95–125	90–130	85–130	85–130

The study was carried out with 22 Anxiety States, 20 Obsessionals, 20 Conversion Hysterics and 40 normal Controls. The psychiatric population were voluntary patients in a number of mental hospitals who volunteered for the experiments. Their selection was primarily governed by the need for homogeneity with respect to symptoms, age and intelligence. Table 3 indicates that this object was resonably well achieved with respect to age and intelligence. A rigorous attempt was made to safeguard the reliability of psychiatric diagnosis in each case. They were patients on whose diagnosis most clinicians would have agreed, to judge by the symptomatic criteria of classification for the three neurotic subgroups which five psychiatrists were asked to give. (Full details of subjects' symptomatology and psychiatrists' diagnostic criteria will be found in Hamilton, 1956.) These criteria show agreement considerably above what one might have expected to find according to some recent estimates (Ash, 1949; Eysenck, 1952). The Control group was

Table 4. *Means and variances*

Test	Controls	Anxiety States	Obsessionals	Conversion Hysterics
Discrimination tests				
Weight, C.D.				
$\bar{x}$	3·75	3·36	1·05	0·95
S.D.	2·32	1·93	1·28	1·41
Length, C.D.				
$\bar{x}$	4·57	5·54	2·30	2·60
S.D.	2·80	3·06	2·37	1·85
Brightness, C.D.				
$\bar{x}$	4·38	5·14	2·20	3·00
S.D.	2·45	3·22	2·38	2·26
Block sorting test				
$\bar{x}$	6·28	7·05	3·10	5·05
S.D.	7·33	7·26	4·62	5·46
'Others'				
$\bar{x}$	18·80	18·73	24·90	17·25
S.D.	11·05	13·45	8·95	11·99
Ambiguous Drawings				
Sheaf-tree				
$\bar{x}$	1·92	1·09	0·55	0·70
S.D.	1·35	1·13	1·15	0·99
House–car				
$\bar{x}$	1·75	0·86	0·05	0·50
S.D.	1·62	0·91	0·22	1·24
Car–dog				
$\bar{x}$	1·77	0·43	0·31	0·80
S.D.	1·57	0·66	0·69	1·17
'Ambiguity Scale'				
$\bar{x}$	8·30	11·55	9·20	11·35
S.D.	3·81	3·03	4·49	3·69
Reversible Perspectives				
Autonomous Revers. Rubin Figure				
$\bar{x}$	12·75	9·95	7·45	6·55
S.D.	9·37	5·57	5·63	5·33
Autonomous Revers. Cube				
$\bar{x}$	11·92	9·63	9·40	6·90
S.D.	5·80	4·38	4·82	5·81
Controlled Revers. Rubin figure				
$\bar{x}$	25·80	17·59	15·65	11·95
S.D.	14·80	11·55	11·14	10·66
Controlled Revers. Cube				
$\bar{x}$	22·52	14·09	16·70	10·95
S.D.	11·56	6·35	8·78	8·18

made up of hospital and college employees of roughly similar ages. Intelligence quotient for the Control group were obtained on the basis of occupations using Vernon's (1949 table of norms.

(iii) *Results of comparative study*

Non-parametric methods were used to calculate the significance of differences betwee the experimental groups. Over-all differences among all groups were calculated by usin a one-way variance analysis by ranks (Kruskal & Wallis, 1952), the results of whic approximate to the χ^2 distribution. Significance of differences between the whole Neu rotics and the Control group, between the Neurotic subgroups and the Control grou and between the Neurotic sub-groups themselves, were calculated by means of th Mann–Whitney U-test (Mann & Whitney, 1947).

Table 4 gives the means and S.D.'s of each subgroup. The following facts emerge (1) The means of the Neurotic subgroups, especially the means of the Conversion Hysteri and Obsessional groups, are generally lower than the means of the Control group (highe in the case of the 'Ambiguity' scale, where a high score is indicative of avoidance o ambiguity). (2) The means of the groups of Conversion Hysterics and Obsessionals ar generally lower than the means of the group of Anxiety States. (3) The means of th groups of Conversion Hysterics and Obsessionals do not appear to differ substantiall from one another. (4) The distributions of the Neurotic subgroups on almost all tests ar highly skewed, whereas those of the Control group show a greater tendency to eithe normality or bi-modality.

Table 5

	A. Over-all differences among experimental groups		B. Differences between Neurotic an Control groups ($m=40$, $n=62$)	
Test	χ^2	P(D.F. = 3)	U	P
Discrimination Tests				
Weight, C.D.	13·1	<0·01	628½	<0·01
Length, C.D.	18·6	<0·001	936	<0·05
Brightness, C.D.	14·8	<0·01	908	<0·05
Ambiguous Drawings				
Sheaf–tree, 'Either'	10·3	<0·02	819	<0·01
House–car, 'Either'	19·7	<0·001	608½	<0·01
Car–dog, 'Either'	2·3	N.S.	644	<0·01
Block Sorting Test				
C.D.	10·1	<0·02	1062½	N.S.
'Others'	5·5	N.S.	1350½	N.S.
Ambiguity Scale	12·9	<0·01	746	<0·01
Autonomous Reversals				
Rubin Figure	17·8	<0·001	761	<0·01
Necker Cube	23·9	<0·001	737	<0·01
Controlled Reversals				
Rubin Figure	20·2	<0·001	653	<0·01
Necker Cube	22·6	<0·001	629	<0·01

Table 5A gives the results of the one-way analysis of variance by ranks. In only tw instances are over-all differences among the four groups not shown to be significan Subsequent individual tests of significance, however, limit the lack of significance show here to specific groups (viz. Table 6). Table 5B gives the results of tests of significan between the data from all Neurotic subgroups pooled, and the Control group. In on two cases is significance of difference contra-indicated by the U-test.

Table 6 gives the results of tests of significance by the U-technique, between all the oups individually. These substantially confirm the differences between Controls and eurotics indicated above. They are, however, more important for the comparisons tween the Neurotic subgroups. These show that, of the thirteen comparisons between ta from the groups of Conversion Hysterics and Anxiety States, six are significant at e 5% level or better. The comparisons between Anxiety States and Obsessionals yielded ven significant results, while the comparisons between Conversion Hysterics and sessionals yielded only three significant differences, one of which—differences on the urth category of the Block Sorting Test: 'To be sorted into other groups afterwards'—as specifically expected, in that this category was included because it was thought differentiate between the classificatory tendencies of Conversion Hysterics and sessionals.

Table 6. *Inter-group tests of significance by U-technique*

(Controls (C) = 40, Anxiety States (A) = 22, Obsessionals (O) = 20, Conversion Hysterics (H) = 20.)

	Groups compared					
Tests	C *v.* A	C *v.* O	C *v.* H	A *v.* O	A *v.* H	O *v.* H
Discrimination Tests						
Weights, C.D.	—	121*	134*	66*	73*	—
Length, C.D.	—	204*	219*	77½*	84*	—
Brightness, C.D.	—	194*	255	102½*	135	—
Block Sorting Test						
C.D.	—	—	—	133	—	—
'Others'	—	—	—	—	—	121
'Ambiguity' Scale	262½	—	133½*	—	—	—
Ambiguous Drawings						
Sheaf–tree	—	216*	258	141½	—	—
House–car	305½	129*	174*	107½*	—	—
Car–dog	210*	180*	254*	—	—	—
Autonomous Reversals						
Rubin Figure	—	127*	156½*	142	111½*	—
Necker Cube	299½	—	159*	—	126½	127
Controlled Reversals						
Rubin Figure	—	196½*	145½*	—	127½*	—
Necker Cube	225½*	265	138*	—	—	110½

* Denotes significance at 1% level or better, plain figures denote significance at 5% level or better. Non-nificant data are omitted.

(iv) *Discussion of results*

Two findings are particularly worth noting in the data of the discrimination tests. ese are the significant differences between the groups of Anxiety States and Obsessionals, d the lack of significant differences between Conversion Hysterics and Obsessionals. e Obsessional group produced the lowest means in two of the three discrimination ts, that is to say they produced the smallest number of 'Can't Decide' responses. view of what has been written about obsessional doubt, it can only be assumed (*a*) that s not operative in situations like those of the tests used here, when a definite judgement o one of three categories has to be made, or (*b*) that it is manifested only in the time en to reach a decision. Alternative (*b*) may be rejected because it was shown in the ot study that there were no significant differences in length of judgement time between ir relatively homogeneous clinical groups including Obsessionals. It would appear

more likely that obsessional doubt does not prevent the individual from making a definit judgement, even if, as in the present case, a correct judgement is objectively impossible Furthermore, there would appear to be grounds for linking avoidance of the middl category of these discrimination tests with the level of overt anxiety. In two of th three tests the group of Anxiety States had a higher mean of 'Can't Decide' scores tha the Control group. There are no grounds for doubting the accuracy of the introspectiv remarks by a number of mainly anxious patients to the effect that they preferred t make a C.D. judgement because they were afraid of making a mistake. By contrast Conversion Hysteric and Obsessional patients reported that they found it relativel easy to reach a decision, and that, in any case, they preferred to decide.

The Block Sorting Test was especially devised not only for the purpose of providing further ambiguous judgement situation, in which wooden blocks incorporating bot rectangular and circular features simultaneously in the same stimulus had to be sorte but particularly in order to attempt to differentiate between the classifying tendencies c Conversion Hysterics and those of Obsessionals. Clinical evidence (Fenichel, 1945 experimental evidence (Rosenberg, 1953), and common everyday knowledge have type the Obsessional as one who is prone to differentiate sometimes on minutiae, and th Hysteric as one who tends to gloss over sometimes quite marked discrepancies in hi environment (Davis, 1948). In the present context that suggested that the Bloc Sorting Test with two middle categories, 'May be circular or rectangular, I can't decid which' and 'To be sorted into special groups afterwards', together with the 'Circular and 'Rectangular' categories, might differentiate between these two clinical groups. Thi was indeed the case. As before, Obsessionals and Conversion Hysterics both tended t avoid the 'Can't Decide' category, but they were significantly differentiated by the scores on the fourth ('Others') category of the test. If, as it appears from the results c almost all the tests of the present battery, Conversion Hysterics and Obsessionals hav specifically marked tendencies to classify their environment, indicated either by the avoidance of the middle categories of a response series, or by avoidance of a response tha might leave them with an excessive amount of subjective uncertainty, the results of th Block Sorting Test would seem to supply some explanation of this similarity of behaviou For it would seem that Conversion Hysterics classify by excessive use of few, mutual exclusive categories, while Obsessionals achieve a similar object by classifying into large number of small categories. Further support for this suggested explanation com from the series of Ambiguous Drawings. These series of amorphous or compounde drawings had to be sorted initially into three categories: 'Object 1', 'Object 2', ar 'May be either Object 1 or Object 2, I can't decide which'. Subjects were then request to subdivide the middle category into a group of 'Eithers' and a group of 'Neither The result was that the Obsessional group put into the 'Either' category all those desig which, given opportunity, they would have classified in the first instance as 'Neithe Object 1 nor Object 2. They clearly would have preferred to split up these designs wi a 'bad fit' into subsidiary groups, just as they had preferred to do in the case of the Blo Sorting Test. The rank correlation between 'Others' in the Block Sorting Test a 'Neithers' in the Ambiguous Drawings was +0·161, which, while falling short of sig ficance at the 5% level, clearly supports the argument.

It may be felt that these findings underline once again the incompleteness of any gene theory of cognition which ignores individual differences in the handling of affect. T

ed to take these into account appears to be barely recognized by Bruner (1956). runer's book, '*A Study of Thinking*', had not appeared at the time of writing. It would em that one of the objects in thinking strategies in concept formation is to minimize rprise or cognitive strain. But these are considered to be functions of environmental es rather than of motivational needs of the individual. Moreover, what Bruner had lled the availability of categories may be determined fully neither by specific cognitive ategies nor principles of cognitive control (Klein, 1954) nor even intelligence, but by dividual differences in willingness to accept an equivocal, undetermined, unstructured d subjectively uncertain situation, which may be engendered when no category for tegorizing environmental cues is available.

On the whole, the 'Ambiguity' scale differentiates less well between the clinical sub-oups. While Tables 5A and 5B show differences for this test to be significant, Table 6 dicates that this finding is mainly an artefact of the low scores of the Control group w scores on the scale being indicative of non-avoidance of ambiguity). Special factors ight possibly account for the results from the groups of Anxiety States and Obsessionals. ie latter score close to the Controls, the former farthest away from them. The scores Convention Hysterics and Anxiety States are also hardly differentiated. This lack of lective sensitivity is not entirely maintained when the items carrying a group's highest ores are examined, because several items are then seen to differentiate very well. rthermore, the groups of Conversion Hysterics and Obsessionals showed a greater nount of inconsistency and contradiction in their responses, which might be endo-nous to their personality organization—reflecting what has been called 'the closeness opposites' (Frenkel-Brunswik, 1951; Rokeach, 1954).

The results of the tests of Reversible Perspective show clear differences between eurotic and Control subjects as already reported by Eysenck (1947). The Conversion ysterics of the present study recorded the smallest number of oscillations both under con-olled and autonomous conditions of the experiment. The difference between them and e Obsessionals is significant in only two cases out of four, while the difference between sessionals and Anxiety States is significant or approaches significance also in two out of ir cases (viz. Table 6). On balance, therefore, a more careful and analytic study might expected to show considerable similarity between the responses of Obsessionals and nversion Hysterics to the Reversible Perspective situation. This expectation derives nforcement from the correlation of these tests with the Discrimination Tests and the nbiguous Drawings, on which a great deal of affinity between these clinical groups was tually demonstrated.

III. Conclusions

attempting to account for the findings, some more obvious explanations must first considered. It has already been shown that age and intelligence are only minimally volved. Though both correlate with some tests that are themselves correlated, these rrelations remain significant after partial correlations have been calculated. Table 3 ows that age and intelligence are as closely matched as possible in an investigation ose prime concern was with obtaining clear-cut representatives of specific clinical oups.

A rather obvious explanation may be suggested in terms of differences in suggesti-ity. While Eysenck (1947) found fairly convincing differences between Neurotics and

Controls, showing the former to be more suggestible than the latter, three objections mu be raised against considering suggestibility as an adequate explanation of consiste avoidance of ambiguity.

(1) There is no evidence to show that suggestion as contained in the body-sway ty of test and as contained in the instructions of the present battery is equally likely arouse compliance.

(2) If suggestion as contained within the test instructions was, however, operativ affecting the Neurotic group more than the Controls, then the responses of the Neurot group to the tests of Reversible Perspective should have yielded higher scores than th in fact did, because in this case suggestions of reversibility of design were quite specifi while they were toned down and controlled in the other tests.

(3) None of the published evidence on differences in suggestibility between Neurot subgroups is conclusive on the question as to which group is more suggestible. Clinic and medical evidence favours Hysterics (McDougall, 1929), Eysenck's evidence (194 favours Dysthymics, without distinguishing between Obsessionals and Anxiety State Neither theory is adequate to explain similarity of performance of Conversion Hysteri *and Obsessionals.*

It may be argued that group differences were due to different levels of aspirati in the subjects. Anxious patients are known to be more highly motivated in terms goal discrepancy scores (Himmelweit, 1947). In the present series of tests this analo is inapplicable. Subjects do not know what constitutes a 'good' performance and the is no knowledge of results. On the other hand, it would be surprising indeed if there we no differences in subjects' attitude to the testing situation or even to the investigato This, however, is a variable that is not easily conducive to measurement, and beside there are no grounds for supposing that it should be found to operate equally in all test

Explanations in terms of differences in perseverative tendencies would also have to of an *ad hoc* kind, because they could apply only to some and not to other tests, wh in some of the situations, viz. Weight Discrimination and Ambiguous Drawings, th possible effects were balanced out. Moreover, Eysenck (1947) found no significant d ferences in perseveration between Hysterics and Dysthymics. This could account f lack of differences between groups of Conversion Hysterics and Obsessionals, but n for differences found here between Anxiety States and Conversion Hysterics a Obsessionals. In addition, there is a further difficulty in that Rim's study (195 indicates that a battery of tests of perseveration was not analysable into a general factor perseveration.

The explanation preferred here is one that is economical, comprehensive, consiste with experimental evidence and in harmony with psychopathology. The clinical grou of this investigation are known to differ with respect to their handling of conflict a anxiety. Conversion Hysterics are known to repress both. Obsessionals are consider to have passed through a phase of repression before obsessive-compulsive symptoms ha been developed in which anxiety is being 'bound'. Such mechanisms have been copiou described as defence mechanisms of the ego (S. Freud, 1893, 1894, 1896, 1915, 1936, et A. Freud, 1942; Brown, 1940; Fenichel, 1945) and reference has already been made them. From their descriptions emerges the picture of the neurotic individual who a need to avoid 'dangerous' situations. Ego defence mechanisms may be conceived o operating in the service of this need. Limitations placed upon behaviour such as obs

ve-compulsive symptoms, avoidance of difficulties, rigid classification of the environ-
ent, scotomization and physical conversion symptoms, which appear in the process of
ontrolling anxiety, would all seem to be designed to reduce the chances of further con-
icts and anxiety having to be faced by the subject. It may be considered an attempt to
aintain the individual in an environment that is predictable, and in which meaning of
imulus and outcome of response are subjectively certain. This quality, however, is
arkedly absent from a conflict and anxiety situation. They are qualities, too, which are
arkedly absent from ambiguous situations. Miller (1951) considers that the develop-
ent of protection against ignorance, i.e. rapid closure behaviour, and the tendency to
hieve environmental stability as demonstrated, for instance, in the 'distorted room
henomena', are factors to lessen fear or anxiety that might arise 'from the constant
reat of an uncertain environment'.

It is in this sense that the situations of the present battery of tests may be perceived as
reatening by some subjects more than by others, for they not only present conflict situa-
ons, but by virtue of their objective lack of definition and structure they may give rise
o subjective uncertainty and therefore anxiety.

These conceptualizations, only partly based on experimental data, would encourage
o more than a tentative formulation of a general principle which would account in
conomical terms for the great majority of the experimental findings on avoidance of
mbiguity, the theoretical considerations bearing on cognitive control, and the known
echanisms of ego defence. This formulation might run as follows. 'Avoidance of
mbiguity as a principle and expression of cognitive control is found in association with
relatively high degree of total anxiety, but particularly where the principal defence
echanism adopted by the individual to cope with anxiety and conflicts is repression.
his mechanism leads the individual to deny reality rather than acknowledge it. It
ecomes generalized to the perceptual field of operation, where by negative methods of
miting and restricting the individual's field of awareness and behaviour, it tends to lead
o the avoidance of responses which might result in uncertainty and anxiety, on account
f the degree of perceptual conflict, equivocality and unstructuredness inherent in such
tuations. By avoiding ambiguity, the Neurotic person, and the Conversion Hysteric
nd Obsessional in particular, would appear to avoid both subjective uncertainty and
onflictful situations. By avoiding uncertainty and conflict, the individual would appear
o avoid further anxiety.'

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FREQUENCY, RECENCY, ASSOCIATION VALUE AND TACHISTOSCOPIC IDENTIFICATION

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Studies of the relationship between cognitive factors and the tachistoscopic identificatio of verbal material (arising out of the failure to control the familiarity of words used i early experiments on personality and perception) have shown that frequency, bot relative (Howes & Solomon, 1951) and controlled (Solomon & Postman, 1952), is inversel related to 'recognition thresholds'. Vanderplas (1953), following gestalt principles, foun that when frequency was held constant organized material was easier to recognize late than non-organized. Other theories have also been applied in this field, e.g. informatio theory (Miller, Bruner & Postman, 1954), and learning theory (Reece, 1954). There sti seems to be room for more exploratory studies, however, especially of the recency facto and of the interrelationships between the various factors. This report describes such study.

Sixteen students (eight men and eight women) acted as subjects. The material co sisted of two groups of nonsense syllables, one drawn from Glaze's (1928) 100% as sociation value list (High) and the other from his 20% list (Low). The syllables, in th order in which they were learned, were as follows:

High: rom, tak, bur, giv, dep, soc, haz, puf, liq, ves.

Low: qus, kig, saj, fep, gok, ruw, jih, xaf, leb, zor.

The factorial design of the experiment involved the variables of frequency (One and Fou presentations), recency (Immediate and Delayed recognition), type of material (Hig and Low value syllables), and sex differences. One subject was randomly assigned t each of the resulting sixteen conditions. As only one list of syllables was learned, bu both were later identified, each subject acted as his own control. Replication was i terms of the material.

The subjects, who had no prior acquaintance with nonsense syllables, came individuall to each part of the experiment. In the learning period, they were asked to memorize th syllables, which were presented in serial order by means of an Elite projector. Eac syllable was exposed for 4 sec. and subjects were told how often they would see then When this was four times, there was an interval of 32 sec. between presentations. Th tachistoscopic procedure, which was the same for all subjects, took place 5 min. or 1 wee later. The 5 min. period was occupied by reading. A projection tachistoscope was use Three practice syllables (col, mic, wef) were presented first to overcome the learning effe of the procedure, and to establish individual shutter speed and initial illumination. Eac of the original twenty syllables, in a pre-arranged random order, was then presented b the ascending method of limits until correctly reported. Exposure speed was constant, an illumination was increased by 2 volts each trial. The lowest illumination at which subject correctly reported any of the syllables was taken as his first trial, and the sco for each syllable was the number of trials required to report it correctly.

No relationship was found between the serial order in which syllables were learned and e scores obtained on them.

Analysis of variance showed that the between-groups' variance estimate was signifi-ntly larger than that for within-groups ($F = 4{\cdot}65$, D.F. = 31 and 288, $P < 0{\cdot}001$), hence ch condition was analysed separately. The mean score for both learned and unlearned oups of syllables was calculated for each subject and the difference tested for signi-ance. No significant differences were found for *single* frequency learning, but all were the expected direction except for two conditions: Low–Immediate–Male, and Low–elayed–Female. For material seen *four* times, significant and nearly significant dif-rences were as follows: High–Immediate–Male ($t = 3{\cdot}2$, $P < 0{\cdot}01$), High–Immediate–emale ($t = 2{\cdot}35$, $P = 0{\cdot}035$); High–Delayed–Male ($t = 2{\cdot}72$, $P = 0{\cdot}015$); Low–Delayed–ale ($t = 3{\cdot}93$, $P < 0{\cdot}01$), Low–Delayed–Female ($t = 2{\cdot}84$, $P = 0{\cdot}01$).

The last two results were the most noteworthy. Why should the influence of learning w value syllables appear only after a week and not a few minutes later? To test the liability of this finding, these two conditions were repeated using eight subjects (four en and four women) in each of the two groups. In addition to the tachistoscopic rocedure, these subjects were presented with the twenty syllables (in a new random der) by means of a drop-shutter exposure apparatus wired to a voice key and chrono-on. Free associations were asked for, and reaction times noted. Allowing 10 sec. for ilures to associate within this interval, the range of mean reaction times for each llable in the high value group was 1·25–2·33 sec., and for those in the low group it was 71–5·64 sec. The corresponding medians were 1·15–1·80 and 1·65–4·30 sec. These gures seem to justify the classification of the syllables as far as these subjects are con-rned. Their scores on the tachistoscopic presentation also justify the earlier results for ese two conditions: for the Immediate group, the mean score for the learned (Low) llables (3·45) was not significantly different from that for the unlearned (High) ones ·50). For the Delayed group, the respective means were 3·45 and 4·81, and the difference as again significant ($t = 2{\cdot}77$, $P < 0{\cdot}01$). A comparison between the scores for the two oups shows that the means for learned material are identical, but that the mean for nlearned material is appreciably higher for the Delayed group than it is for the mmediate. This difference is, in fact, significant ($t = 2{\cdot}95$, $P < 0{\cdot}01$).

One way to explain these results is to suppose that the learned syllables had become rganized into a conceptual system, and that the set to perceive nonsense syllables ubjects were merely told that nonsense syllables would be flashed at them) had aroused is system and served to retard identification of non-members. It was found in a study y Postman & Leytham (1951), for instance, that when a series of tachistoscopically resented trait names was unexpectedly followed by a noun (matched for familiarity), e number of trials required to identify the noun was greater than it was for the first, on-trait, practice word. The reinforced trait set seems to have delayed the identification the noun. If, as suggested, the same process operated in the present experiment, why as it not apparent when the test was carried out immediately after learning? One ossible explanation is that it takes time for material of low association value to become rganized into an effective conceptual system: an incubation period, rather like that ssociated with creative thinking, is required.

A consideration of how subjects arrived at correct responses in this study, suggests at, for nonsense syllables at least, there are roughly three stages in this process:

(1) seeing the stimulus (sensory threshold), (2) recognizing the stimulus (perceptual recognition threshold), and (3) remembering what has been perceived long enough to repo or record it. Inability to report a stimulus correctly may thus be due to factors operatin at any one or more of these stages, and if this process is general for verbal material, suc terms as 'perceptual defense' and 'subception' may beg the question. It is for th reason also that the term 'tachistoscopic identification' (Miller *et al.*, 1954) is to b preferred as a description of a subject's correct verbal response in this kind of experimen

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THE PREDICTION OF EXAMINATION MARKS OF MECHANICAL ENGINEERING STUDENTS AT KING'S COLLEGE, NEWCASTLE

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The correlation coefficients between the examination marks of 108 University engineering students and various predictors are given. It is concluded that School Certificate, G.C.E. (Advanced Level) or Higher National Certificate (A1) results provide the best prediction of engineering marks.

I. Introduction

his paper is concerned with the prediction of the examination marks of mechanical ngineering students at King's College, Newcastle. Hohne (1955) and Sanders (1948) oth concluded that Australian school-leaving examination results gave the best predic- on of first-year University engineering marks. A useful summary of the literature egarding University selection in general is given by Dale (1954).

II. Method

ne sample consisted of 108 students who had entered King's College in the sessions 1952–3 to 1954–5 clusive. Some of these students had taken the first-year examination, whereas some had taken the cond-year and the remainder had completed their final examinations. The correlations between the ree examinations were calculated, and it was found that the second and final examinations correlated ghly (0·86), whereas the average correlation between the first and subsequent examinations was only 38 (Fisher's z method). It was therefore decided to adopt the second or the sum of the second and final amination marks as one criterion and the first examination marks as a separate criterion. Validities of e predictors against these two criteria were then calculated.

The predictors consisted of the ratings of four interviewers, the results of N.I.I.P. tests 90A (verbal) d 100A (verbal and non-verbal) and of Peel's test V.S. 23 (non-verbal), School Certificate examination sults, average G.C.E. (Advanced Level) marks or average A1 marks of the Higher National Certificate amination. Only first attempts at the three examinations were considered. The Advanced Level bjects included pure and applied mathematics, physics and chemistry only.

The School Certificate results were scored by awarding 3 marks for a distinction (or a very good), marks for a credit and 1 for a pass. All subjects were taken into account. Two additional marks were arded for every year by which the candidate was younger than 15 years when he took the examination. vo marks were deducted for every year the candidate exceeded 16 years when taking the examination.

Three of the interviewers (A, B and C in Table 1) interviewed the candidates as a panel and had hool Certificate, Advanced Level and A1 results to refer to. The remaining interviewer (D), however, ied entirely upon the data obtained at the interview which he conducted by himself.

III. Results

ne product moment correlations between the predictors and the criteria are given in able 1. Except where shown, $N = 33$ for the first examination validities and $N = 94$ for e second and final examination validities.

Both the average validities of the School Certificate, Advanced Level and A1 results e greater than any other validity, but not significantly so in every case. When, however, ese two average validities are themselves averaged, then the resulting average validity ·54) is significantly greater than the average validity of any other predictor.

Table 1

Predictor	Validities: Against 1st examination		Validities: Against 2nd and final examination	
Interviewer A	0·370*		0·335**	
Interviewer B	0·242		0·318**	
Interviewer C	0·046		0·355**	
Interviewer D	0·379*		0·179	
Test 90A	0·361*		0·071	
Test 100A	0·315		−0·006	
Test V.S. 23	0·103		0·052	
School Certificate	0·550** ($N=22$)	Average 0·578**	0·378 ($N=26$)	Average 0·532**
Advanced Level. G.C.E.	0·727 ($N=6$)		0·688** ($N=17$)	
A1, H.N.C.	—		0·561** ($N=27$)	

** denotes a highly significant correlation ($P<0·01$); * a significant correlation ($P<0·05$). The difference between the School Certificate, Advanced Level and A1 validities are not significant.

IV. Discussion

The results obtained indicate that School Certificate, Advanced Level or A1 result provide the best prediction of the engineering marks in question. This conclusion is i agreement with those of Hohne (1955) and Sanders (1948); their correlation coefficient between first-year engineering marks and the Australian school-leaving examinatio were about 0·7, whereas their cognitive test coefficients were about 0·5. It is likely tha the second and final examinations are reached by only those students who are inte lectually capable. This may explain, at least in part, the vanishing second and fina examination validities of the three tests. The low interview validities suggest that th interview is of doubtful value as a predictor of University engineering marks. This con clusion is in accordance with previous findings regarding University selection in genera

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SHAPE RECOGNITION IN RATS

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Reasons are given for doubting whether certain of Lashley's experiments, purporting to be investigations of shape discriminations, really are such. In particular his finding concerning the square-circle discrimination cannot be held to lend support to Deutsch's theory of shape recognition. An experiment is reported which shows that rats can discriminate square from circle under certain conditions of pre-training, and do so under these conditions by responding to the horizontal base of the square; the response is transferred to other shapes having a horizontal base, but not to shapes lacking it. There is some evidence to show that this happens for small shapes—where Deutsch's system could be operative—and this is therefore evidence against his theory. A theory of shape recognition is proposed of the same logical type as Deutsch's, which accounts for the results here reported, accords well with other known characteristics of shape recognition in primitive visual systems, and presents less difficulties than Deutsch's system as a possible neurological model.

I. Introduction

A well-known paper by Lashley (1938) sets out some of the basic characteristics of vision in rats. The purpose of the experiments he reports was to establish norms of behaviour, rather than to prove any hypothesis or support any particular theory of vision. One of Lashley's findings, apparently quite general in the experimental situation used, was that the rat tends to isolate from a complex visual stimulus an element, or elements, to serve as the basis of discrimination. The animal uses only part of the information available. This was noted in shape discrimination; under conditions of equal area and brightness, shapes tended to be discriminated from one another by isolation of an element or property present in only one of the shapes. It was also found that the most important cue in this type of discrimination is the relation of the part-figure to the edge of the stimulus card: 'Systematic tests have shown that the majority of animals do not react to the entire figure presented in the stimulus patterns.... The most important factor in defining the part-figure is the relation of the stimulus card to the surrounding frame.' (p. 152.)

Further exposition will be simplified by the definition of three terms: (i) 'stimulus shape' is the pattern drawn on a stimulus card; (ii) 'stimulus card' is the card presented in the jumping stand (Lashley, 1938 p. 126); (iii) 'discriminated shape' is the part of the stimulus situation to which the rat is responding. For (i) and (ii) simply 'stimulus' and 'card' will be used when the context makes the meaning clear.

There are several important features of Lashley's experiments which make their interpretation difficult, especially so far as shape recognition is concerned. (*a*) The rats were pre-trained to discriminate between horizontal and vertical striations. (*b*) The ratio of stimulus shape to stimulus card was large; that is to say, the internal dimensions of the shapes were of the same order of magnitude as the distances between shape and card edges. (*c*) The stimulus shapes presented quite a large visual angle at the rat's eye. The peculiarities mentioned above, concerning part-figure discrimination and discrimination by the relation of stimulus shape to card edge (the latter I shall call a 'boundary effect')

may be artefacts of the conditions of the experiment, rather than characteristics of shape discrimination.

It is by no means certain, then, that Lashley was investigating shape discrimination as such; especially as many of the figures used were solid figures, rather than outline figures, it appears possible that the rats were responding to the distribution of light on the stimulus card, and not to shape. (This point will be further elaborated below, in §III.) Even if they were reacting to shape, the discriminated shape was not in general the stimulus shape, or even a part of it, but some relationship between the stimulus shape and the stimulus card.

In a recent paper (Deutsch, 1955), a theory of shape recognition was put forward by Deutsch, which accounts for Lashley's rather surprising finding that rats have great difficulty in discriminating a square from a circle of equal area and brightness. This theory, as it stands, requires that a shape, to be recognized, must all be seen at the same time, or, to put it more precisely, each part of the discriminated contour must excite a separate receptor, or group of receptors, simultaneously. (The division of the contour into separate parts presents no difficulty, so long as the number of parts is small compared with the number of available receptors. In mammalian eyes, at least, this condition would appear to be fulfilled.) It seems plausible to suppose that the size of a discriminated shape will be restricted by the size of the rat's field of distinct vision; this field, according to Lashley, is small, although its precise extent is not known.

It appears, then, that Deutsch's system could not operate in the conditions of Lashley's experiments, and the anomaly concerning square-circle discrimination which it accounts for so neatly may, in this case, have a quite different explanation. In view of the objections listed above it would be desirable to know whether the supposed inability of the rat to discriminate square from circle holds under conditions in which Deutsch's system of shape recognition could function. This can be done in two stages; the first is to eliminate, as far as possible, the boundary effects, and to find out whether (*a*) the inability to discriminate square from circle still obtains for stimulus shapes of the size used by Lashley under conditions of pre-training similar to his, and (*b*) how this discrimination is affected by reducing the size of the stimulus shapes. The second stage consists in an investigation of the discrimination of small shapes without pre-training in any form of shape discrimination.

The experiment reported below deals only with the first stage, but it is hoped that in due course experiments on the second stage, at present in progress, will be reported.

II. The experiment

The purpose of the experiment was to investigate the discrimination of square from circle in the rat, under conditions similar to Lashley's, but avoiding as far as possible the introduction of boundary effects; to investigate the effect of different sizes of stimulus shape on this discrimination; and to determine to what extent pre-training on horizontal and vertical striations affects subsequent discrimination.

A modified form of Lashley's two-card jumping stand was used; the important modifications were: (i) A Y-shaped jumping platform, with arms set at 45° to each other; (ii) stimulus cards placed at 135° to each other, each card thus facing one of the arms of the platform at right angles; (iii) large black stimulus cards, 11 in. square, each card containing a 4 in. door in its centre, fitting flush with the card; (iv) stimulus shapes, cut out in black cardboard, could be attached to either stimulus card by a simple device; (v) each stimulus shape was outlined in white ink. The advantages which these modifications give will be obvious; it is not claimed that the arrangement described in (iii) which is intended to abolish, or reduce, boundary effects makes the shape completely isolated from the edges of the card, or that the inside edges of the

ard surrounding the door are invisible. It is an improvement, however, on Lashley's original apparatus, o far as investigating shape discrimination is concerned. The stimulus shapes were 'outline' rather than olid' figures, as with the latter discrimination might be based simply on the distribution of brightness the stimulus shape (see §III below). In Deutsch's theory, which deals only with contours, the two ypes of figure are equivalent.

The shapes used were squares, circles, triangles and pentagons. There were five sizes for each shape, he squares ranged from side $1\frac{1}{2}$ in. (size 1) to side $3\frac{1}{2}$ in. (size 5), by $\frac{1}{2}$ in. steps, and the sizes of the other hapes were determined by equating them for area, in each case, with one of the squares. Thus there as a series of five sizes for every shape.

During training shapes of equal area were always used: however, there are three cues which might ill be used as a basis for discrimination, if all the outlines are of equal width. They are: (i) circumerence length (which differs for different shapes of equal area); (ii) total brightness; and (iii) what may be alled 'subjective' size.

To eliminate (ii) several copies of each size of the shapes were made, each having a different width of utline, and hence differing in total brightness. These were interchanged during training, from time to me, so that total brightness would not serve to distinguish the shapes; the difference in brightness etween the shapes was in any case small. Control of (i) and (iii) was effected by showing different *sizes* f the training shapes together after discrimination had been established; in no case had discrimination een based on either of these cues.

The subjects were ten hooded male rats from the colony kept at the Institute of Experimental Psyhology at Oxford. (In fact twelve rats were used, as originally reported (Dodwell, 1956), but the results r the other two, although they support the general conclusions here given are not reported as the exerimental conditions for these two animals differed in certain respects from the conditions for the rest f the experiment.) At the start of the experiment they were approximately 100 days old.

All the animals were put on a 24 hr feeding schedule (being fed for 2 hr. in 24) a week before the xperiment began, and were given opportunity to become familiar with the apparatus. They were then aught to discriminate a wooden door in a black card from a plain black card, and this situation was ontinued until jumping was well established.

Next, discrimination of horizontal from vertical striations was learned. In this, as in the subsequent iscriminations, the criterion of success was twenty consecutive errorless jumps; the shapes were changed ndomly, and the rats not allowed to correct errors (thus a wrong jump left would not necessarily mean hat at the next jump right would be correct). Each animal had ten jumps per day. Each correct jump recorded as a trial, each incorrect jump as an error.

On the basis of their performance in this discrimination the rats were divided into three groups (of 4 and 2) roughly equated for number of trials, and errors made.

The training given to each group, together with results, are shown in Table 1.

Table 1

Group	Discrimination		Size of shape	Trials†	Errors†	Criterion reached	Transfers to other sizes
	Positive	Negative					
I*a**	Square	Circle	5	20	0	Yes	4, 3
I*b**	Square	Circle	1	154	56	Yes	2, 3, 4, 5
II	Diamond	Circle	1, 5	164	143	No	
III*	Circle	Square	5	150	165	No	—

* Two rats only.
† Group averages.

Groups I and II were divided, two rats in each being trained on large shapes and two on small. Group I*a* umped without further training to the large square, group I*b* required considerable training before eaching the criterion, but then transferred to shapes of all the larger sizes. There were no differences ithin group II.

Circumstances beyond the experimenter's control made it impossible to continue the experiment with roups II and III for as long as would have been desirable, but the results quoted make it clear that, nder the given conditions, the discrimination of square from circle is possible, and is dependent on re-training on horizontal and vertical striations. It would appear probable that the differences between roup I on the one hand, and groups II and III on the other, can be explained in terms of the horizontal ase-line of the square, since this is an obvious characteristic of the positive pre-training shape which is

present in the positive shape for group I but not for groups II and III (group III showed an initial pre ference for the square also): having learned the horizontal/vertical discrimination the rats transfer t another shape with a horizontal base line. This is confirmed by a series of transfer tests given to group after the criterion on the square/circle discrimination has been reached.

Table 2 gives the transfer shapes and results; in the third and fourth columns are the numbers of correc jumps for individual animals. The sizes of shapes shown were the sizes on which the animals learned, an triangles and pentagons were shown with one side (base) horizontal. Each test consisted of a series o ten jumps followed by ten jumps on the the square/circle discrimination.

Table 2

Positive	Negative	I*a*	I*b*
(i) Triangle	Circle	10, 10	10, 10
(ii) Pentagon	Circle	10, 10	10, 10
(iii) Square	Triangle	5, 5 (position habit)	9,—
(iv) Square	Pentagon	5, 5 (position habit)	10, 10
(v) Square (large)	Square (small)	7, 8	6, —
(vi) Square	Diamond	8, 7	9, 10

There were no results for one animal on (iii) and (iv) since discrimination broke down before the end c the series. With this one exception the square/circle discrimination was maintained throughout the serie During transfer tests both doors were left unlocked, and the shapes changed in a random sequenc It might be supposed that the remarkable transfer in (i) and (ii) was due to simple avoidance of the circle this was not so, as a circle shown with a diamond, an inverted triangle or pentagon gave chance scores, t gether with the typical conflict symptoms of micturition, squealing and long delays in jumping. Transfe (iii) and (iv) are the only ones which differ markedly for the two groups: apparently group I*a* treat an shapes with horizontal base-lines as equivalent (on transfers (iii) and (iv) both animals jumped to th 'preferred side' irrespective of the shapes shown, but both returned to correct discrimination when squar and circle were shown together), but there is a suggestion that group I*b* differentiate square from triangl and pentagon with base horizontal.

The most striking conclusion to be drawn from this experiment is that discrimination has evidentl been achieved by the isolation of an element of one of the shapes; there can be no doubt that this commo element is the horizontal base line. The isolation of this element occurs during the pre-training on hor zontal and vertical striations, and explains the initial preference of groups I*a* and III for the square.

The following detailed conclusions can be drawn, some of them only tentatively, since the numb of animals used was small: (1) Pre-training on shape discrimination has a very marked effect on subs quent shape discrimination. (2) Under the given conditions square and circle can readily be distinguishe by the isolation of an element in one of the shapes; the rate at which this discrimination is learne depends on the size of the shapes, and possibly also on the size of the pre-training shapes. (3) There transfer to a variety of shapes which have the isolated element in common, and this effect persists—a least to some extent—for small shapes. (4) There is no confusion of square with diamond, or of eith of these with circles.

These results clearly have important implications for Deutsch's theory of shape recognition.

III. Discussion

Deutsch (1955) lists six main facts which a theory of shape recognition must account fo Briefly, these are that shape recognition can be independent of: (*a*) localization or fix tion on the centre of the figure; (*b*) the angle of inclination of the figure in the visual fie (rotation on sagittal axis); and (*c*) sizes. Furthermore: (*d*) mirror images are confuse (*e*) visually primitive organisms find it hard, if not impossible, to distinguish square fro circle; and (*f*) shape discrimination survives the destruction of large parts of the stria cortex. For a discussion of these six facts, the reader is referred to Deutsch's pap (Deutsch, 1955, pp. 30–1). The system proposed by Deutsch does satisfy these six chara teristics, but whether they are all characteristics of shape discrimination in visual

primitive organisms, such as the rat, is open to doubt. In particular, facts (*b*) and (*e*) are questionable.

Several investigators have found that a slight rotation on the saggittal axis of a two-dimensional shape interferes with its recognition (Fields, 1935; Gellerman, 1933). Fields puts the threshold for this type of disturbance at between 5° and 10° of rotation for the rat. In this respect, Deutsch's system represents a higher capacity for visual abstraction than that of the rat, or even the monkey (Gellerman).

Fact (*e*) is stated as a characteristic of the visual system of the rat on the basis of Lashley's experiments (Lashley, 1938). It has been pointed out above that there are several serious objections to taking Lashley's experiments as investigations of shape discrimination. Even if they were genuine shape discrimination experiments, the stimulus shapes used were too large to be 'seen whole' and therefore his results are irrelevant to Deutsch's theory; this applies to the finding concerning lack of discrimination of square from circle. (It should be mentioned that failure to discriminate square from circle has been observed in the octopus (Boycott & Young, 1956) but these authors have their own explanation for this).

The results of the experiment described above show that square and circle can be distinguished from one another. There are two important differences between the conditions of this experiment and Lashley's: boundary effects have been reduced, if not eliminated, and the shapes used are 'outline' rather than 'solid'.

That *reduction* of some cues (boundary effects) should facilitate a discrimination may seem puzzling. The use of 'outline' rather than 'solid' figures also involves a reduction of cues, and these are cues of a type which might well interfere with shape discrimination as such; it seems likely that the explanation of the difference between Lashley's findings and mine lies here. It is well known that brightness is a 'dominant' cue for the rat, and it could well be that the use of 'solid' shapes predisposes the rat to seek, if not differences of brightness between figures, different distributions of brightness within figures. Thus a rat might distinguish between a (solid) square and a triangle by distinguishing 'bright above and below' from 'bright below'. It would not be possible to discriminate a square from a circle on this basis using above-below or right-left comparison's or comparisons of one or more quadrants in one figure with the same quadrants in the other. It seems plausible to suppose that attempts at discrimination of this sort would be less likely to occur when 'outline' figures are used against a dark, homogeneous, internal and external background. If Lashley's rats were attempting to discriminate on the basis of brightness distribution, their failure is understandable.

However, it remains to be seen whether rats fail to make the square-circle discrimination under conditions in which Deutsch's system could operate. Data for group I*b* reported above suggest that they can make it, but the only really satisfactory test would be in a situation where the rats had had no pre-training on shape discrimination of any sort.

Whether or not under these conditions the rat can distinguish square from circle, there are two objections to Deutsch's theory which are of a more general character. The first has been mentioned already; the system allows of recognition of a shape in any orientation, but this does not appear to be a characteristic of primitive visual systems; the second is, that it does not allow for discrimination of part figures, or the isolation of part of a *continuous* contour, although apparently it is just this sort of 'abstraction' that mediates shape discrimination in the rat. This would not be a serious objection for

large figures, but the same process apparently occurs with small figures. Group I*b* (se above) treated several shapes with horizontal base lines as 'positive' although there some evidence to suggest that when such figures are shown with a square the square preferred (transfers (iii) and (iv)).

There follows a suggested system for shape recognition of the same logical type Deutsch's but which accords better with the known facts—especially those mentione in the preceding paragraph—and also satisfies the remaining four parts of Deutsch specification (facts (*a*), (*c*), (*d*) and (*f*)).

IV. The theory

Consider a two-dimensional array of units (or cells) arranged in chains, in such a way tha each unit is connected to two neighbours, except for the end units of each chain, each which has only one neighbour. Each chain is functionally separate from every oth chain, except in two important respects: one set of end units (set A) is so arranged tha if one fires, they all fire simultaneously, and the other set of end units (set B) discharg into a common channel, which, following Deutsch's terminology, is called the fin common cable (f.c.c.) Each chain has the same number of units (this is not a necessar condition of the working of the system, but its introduction simplifies matters conside ably). Each unit is connected to a receptor, or group of receptors, and when the recept or group of receptors is stimulated this activates the unit to which it is connected. Suc a unit is 'active'; when a unit is not so activated it is 'passive'. Adjacent receptors exci adjacent units. It is assumed, as in Deutsch's theory, that only excitation from regio of sharp change in stimulation (contours) arrives at the plane of units.

Any unit of the array can pass excitation from itself to a neighbouring unit in the sam chain: this excitation originates in the firing of the end-set A, and is not to be confuse with activation by stimulation of receptors to which the unit is connected. The passi of this excitation from one unit to another involves a small delay, the length of which determined by whether the unit transmitting excitation is active or passive. Only t two states are envisaged, and the delays for each are constant,; say Ta and Tp. F convenience, suppose $Ta > Tp$.

The delay involved in excitation passing from one end of a chain of $n+1$ units to t other, when all units are passive, is nTp. The time taken to traverse any particul chain will depend on the numbers of active and passive units in the chain and can written in the form $(n-r)Tp+rTa$. If all the units of the array are passive, the traver times for all the chains will be equal, and there will be a single, large discharge in t f.c.c., since all the A set fire together. The signal in the f.c.c. will depend only on t number of active and passive units in individual chains, and not on the positions of tho chains, or the positions of the units on the chains; this signal can be written simply as t sum of a number of terms of the type $(n-r)Tp+rTa$. Chains with equal numbers active units will fire simultaneously into the f.c.c.

It is assumed that the array is so arranged that chains will represent, functionally, o of the principal axes in visual perception: thus stimulation of groups of receptors by, sa a horizontal line will fire units all on one chain, but which chain this is (i.e. the positi of the horizontal line) will be immaterial as far as the signal in the f.c.c. is concerne which will be a single large discharge (from all the 'empty' chains), followed after a del

a single small discharge (from the one delayed chain). Such a system will be sensitive small changes of direction; a small rotation from the horizontal will throw the activaon off a single chain, and involve fewer units per chain on a *number* of chains. This means at the final signal now is a single large discharge (but smaller than in the previous case) llowed after a shorter delay by a simultaneous discharge from several chains. The system obviously most sensitive to changes of direction near the horizontal. The system will scriminate between simple geometrical figures, seen separately, such as a square and a angle; both will give a signal consisting of three volleys: the first for the 'empty' ains; the second for the non-horizontal sides of the shapes, of high amplitude but short lay, caused by a few active units on a large number of chains; the third for the horizontal les, of low amplitude but relatively much longer delay, caused by a large number of tive units on a small number of chains. The main difference between the two signals that the square's third discharge has about twice the amplitude of the triangle's, since e square has two sides in the horizontal and the triangle only one. Similarly, other nple shapes give rise to distinctive signals. However, as it stands the system would ve difficulty in distinguishing between two rectilinear shapes neither of which had a le in the horizontal: the signals for a diamond and a pair of vertical lines, for instance, ould be practically identical. This difficulty is overcome by assuming that there are o such systems in action, the second one functionally perpendicular to the first, i.e. *vertical* line will stimulate receptors connected to units on a single chain; each system s a separate final common cable.

This double system has the interesting property that horizontal and vertical lines will be scriminated from one another much more readily than mutually perpendicular lines set 45° to the horizontal. This effect has been observed in the octopus (Sutherland, 1957). The system will account well for the experimental results reported above. One would pect shapes to be discriminated by presence/absence of a rectilinear contour lying in ie of the principle axes if this cue is available in a particular case, since this gives a stinctive signal in the f.c.c., and that if this cue has been used as a basis for discrimina-on *any* figure having the property will be discriminated from any figure lacking it. The sults reported above, though limited to a few shapes, are consistent with this.

In this particular case the pre-training figures were large, so the conditions favour olation of an element of the shape. It remains to be seen if the effect persists under less vourable conditions of training.

The difficulty in discriminating small square from small circle (group I*b*, see above) uld be explained either by supposing that a short line produces a smaller delay in the .c. which is less distinctive than the long delay, or by supposing that with the smaller apes more elements of the shape fall within the effective field of vision and hence introice additional signals in the f.c.c. which tend to mask the delayed discharge on which scrimination is based. These are real alternatives, in the sense that an experiment could set up to decide between them.

It is also perhaps worth mentioning that many of the results reported by Lashley can accounted for by the system. To show this in detail for all Lashley's figures would quire a separate paper, but two examples will illustrate the point.

(1) Rats trained to discriminate horizontal against vertical striations transferred to single rizontal against single vertical lines and interrupted horizontal against interrupted rtical; interrupted striations, horizontal against vertical, so long as the interruptions

were small compared with the width of the striations (Lashley, 1938, p. 147). It is particular interest to note that there was no transfer to horizontal and vertical zigza and 'the same result was obtained in other tests with narrower lines and with wa lines, *whenever the deviations from a straight line exceeded the width of the striations*' (19 p. 148) (my italics). This is just the condition which would determine the disruption the distinctive signal in the f.c.c.

(2) Comparison of two of the 'interference' situations shows that patterns which p duce regular interference in the f.c.c. are more disruptive of a discrimination than th which produce 'random' interference. Thus the discrimination of triangle from cross not disturbed by introducing a number of small isolated patterns around the discrim anda (Lashley, 1938, pp. 140–1, figures 20 and 21) but introduction of horizontal a vertical striations (figure 22) does disrupt a discrimination (see also 1938, p. 146). T would be expected: if one regards the f.c.c. as an information channel, the introduction a certain amount of noise will interfere less with the reception of the signal than t superposition of a second signal on the first. (The introduction of a number of sm isolated shapes into the display will generate something close to random noise in the f.c.

It is not claimed that all Lashley's results can be so neatly explained by the theory, n is this to be expected, since other factors besides shape recognition entered into experiments.

The system has the advantage that it will explain equally well discrimination by pa figures or whole figures, and a discrimination need not break down in going from one the other, provided the same distinctive signal (or part of a signal) is set up in the f.c in both cases; in Deutsch's system the signal set up in the f.c.c. would alter radically ev if only a small part of a figure fell outside the field of vision. It also avoids the somew artificial restriction, necessary for Deutsch's theory, of 'numbering' shapes if seve are seen simultaneously (Deutsch, 1955, p. 35).

The limited scanning process required in this theory is free from the objections to su processes in general made by Deutsch (1955, p. 31).

Some of the predictions which can be made from the theory are as follows: (i) Wh *small* shapes are used, there should be confusion of upright with inverted triangle, b not with triangles in any other orientation, and in particular not with a triangle hav one side vertical, although this shape would itself be confused with its mirror ima (ii) The prediction of lowered threshold for discrimination of the angle of inclination straight lines near the horizontal has already been made: it applies equally to straig lines near the vertical. (iii) A discrimination would be upset, not only by rotation of t shapes, but also by rotation of the animal's receptors (e.g. by tilting the head to one si but would be restored by rotating both the receptors and the shapes by equal amou and in the same direction. The testing of this prediction would present practical difficult but it is possible in principle (Gellerman taught chimpanzees and children to discrimin a triangle from another shape; when shown a *rotated* triangle both rotated their he so as to bring the triangle into the learned orientation (base-line horizontal) bef making a discrimination (Gellerman, 1953, pp. 13–17).)

The theory fits the acceptable parts of Deutsch's specification (see above, §III), a also the characteristics of shape recognition found in the experiment reported in t paper. It has plausibility as a system which could be neurologically embodied; in t respect it presents less difficulties than does Deutsch's system, despite the fact t

ocesses of spreading excitation in nervous structures of the type required for Deutsch's eory have been discovered (Burns & Grafstein, 1952; Motokawa, 1951). The theory is stable.

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PUBLICATIONS RECENTLY RECEIVED

Behavior Theory and Conditioning. By K. W. Spence. Yale University Press; Londo Oxford University Press. 1956. Pp. vii + 262. 36*s*.

This volume presents the Silliman lectures at Yale for 1956. Prof. Spence is the first psychologist deliver these well-known scientific lectures, though following physiologists such as Sherrington (T Integrative Action of the Nervous System) and Granit (Receptors and Sensory Perception). It must ha been no easy task to present psychology in this company and criticism from some quarters was inevitab Prof. Spence has chosen to present in an introductory lecture a brief historical survey of psychology a then settle down in five further lectures to examine rat psychology and conditioning. This is a thorough workman-like effort—anything but a lecturer's *tour de force*—and the student with 'Spence' in one ha and 'Hilgard and Marquis' in the other can weigh the evidence in the field of conditioning for hims With such a wealth of detail it would seem invidious to select individual points for criticism. Suffic to say that Prof. Spence, as we might expect from so eminent a Hullian scholar, gives an authoritat presentation of the work of the behaviour theorists and at the same time concisely states the differen between Hull and himself. His readers should be able to make their own assessment of the positi reached by this research for these lectures represent one of the clearest expositions which we have be given of behaviour theory.

In the last chapter on 'Complex Learning' Spence examines the situation where the rat has to cho between one of two bars in a double-type Skinner box and, to finish on a human note, there is a br consideration of how paired-associate learning is influenced by anxiety levels. The author's approach epitomized in his observations that though he is concerned with simple learning phenomena he suspe that 'as yet, we have hardly begun to uncover all of the complexities that underlie even such sim learning behaviour as is involved in these situations.' The reader who has tracked his way through de vatives of Hullian learning formulae and some fairly advanced mathematics may begin to feel rest but there are two points which he should consider. The behaviourists are attempting to derive a interrelate empirical laws from classical and instrumental conditioning. They believe, rightly or wrong that such laws would serve as a theoretical model to account for further complex learning. With su aims the behaviourist is right to attempt to understand the simple situation—it is no criticism of h that he finds it to be complex. Rather he is to be commended for his honesty. At the same time industry and integrity is no guarantee that he is pursuing the right road. We can of course go east going west. But psychologists who object to this course might examine their own alternatives. This Prof. Spence's second point. He pleads—and how much the reviewer agrees with him—for psychology 'to learn to make more effective use of theories instead of taking the negativistic attitude that theor are something to be proved wrong by fair means or foul'. For many of us who see the psychology civil wars as a period of furious and too often misdirected activity a more constructive and more toler phase seems long overdue. Creative research has no time to carp.

H. F

Interpersonal Diagnosis of Personality. By Timothy Leary. New York: The Ron Press Co. 1957. Pp. xix + 518. $12.00.

In this important book Dr Leary describes a 'functional theory and methodology for personality eva tion' which has been developed and applied by the Kaiser Foundation Psychology Research Project which, latterly, he has been the director. In the Preface he describes the approach used as a dyna behaviourism: 'There are two dynamic attributes. The first refers to the impact one person has or ma in interaction with others; the second refers to the interaction of psychological pressures among different levels of personality. The behaviouristic attributes of the system derive from the procedur viewing every response of the subject (overt, verbal, symbolic) as a unit of behaviour which is classi by objective methods and automatically sorted into the appropriate level of personality. The patte and clusters of thousands of these responses, sorted into different levels, are then converted by ma matical techniques into indices and into a multilevel diagnostic code summary. They are then relate clinical events or prognoses.'

In the early chapters the author draws on a number of psychoanalytic sources, notably Sullivan and Erickson, and on a wide range of clinical experience, to formulate nine working principles as a basis for the method. It is stressed that the variables to be used are to be capable of measuring adjustive as well as maladjustive aspects of personality, that they are to permit the classification and measurement of data from different levels of behaviour, and that the final objective is to make possible predictions about interpersonal behaviour in a functional setting, in this case a psychiatric clinic.

Sixteen interpersonal dimensions of personality are used to classify the data. These are ordered on a circular two-dimensional continuum, in which a vertical axis represents dominance-submission, and a horizontal axis, hostility-affection.

Five levels of personality, defined according to the source of the data are described: Level 1, *Public Communication*, concerns the interpersonal impact of the subject upon others. Level 2 is represented by the subject's *Conscious Description* of himself and others in his world. Level 3 derives from the autistic projective fantasy productions: for example TAT, and is called *Preconscious Symbolization*. Level 4, the *Unexpressed Unconscious*, is defined by the subject's omission and avoidance of significant interpersonal themes. Level 5, the *Ego Ideal*, comprises the subject's statements about his interpersonal ideas, standards etc.

It is emphasized that data from any one level can only be understood in the light of data from other levels of behaviour. Illustrations are given, from clinical and other sources, to show the usefulness of the assessments obtained at each level, and of discrepancies between the levels, and between self-assessments and the assessment of others.

In the second half of the book the application of this approach to interpersonal diagnosis is further developed and methods of quantifying the data and of measuring the variability between levels are elaborated. Clinical and research data are reported in detail for eight personality types, which are categorized, on the basis of levels 1 and 2 information, in terms of their dominant interpersonal theme.

Finally some applications of the interpersonal system are described; for example to the diagnosis of hospitalized psychotics and of psychosomatic personality types in medical practice, to the analysis of group dynamics in an industrial management group, and to the prediction and the measurement of interpersonal dynamics in group psychotherapy. Valuable appendices give information about the construction and standardization of the instruments employed in the research, together with norms, conversion tables and weighted scores.

This brief sketch of some of the contents of the book cannot give any proper impression of its richness in ideas or of its breadth of concern. It is an exceedingly important contribution to the psychology of personality, and in particular to clinical psychology. In many ways the approach to personality study and to the research problems involved in it are in the tradition of H. A. Murray's 'Explorations'. The Kaiser Foundation Research may well have a little influence on clinical and social studies during the next decade.

Dr Leary makes it quite clear that he is well aware of the incompleteness of the achievement, particularly in the inability so far to integrate level 4 and level 5 data into the system. Many psychoanalysts will feel that the research has been concerned too much with the development of an ego psychology, and too little with the unconscious dynamics and the ways in which they determine the quality of interpersonal relations. Such shortcomings, however, should not take from the value of this pioneer work. Apart from the practical usefulness of the concepts and methods described, much has been achieved towards 'bridging the ancient and intolerable gap between the science of personality and the practice of psychiatry,' and towards the development of a unified theory of personality.

H. PHILLIPSON

Prediction Methods in Relation to Borstal Training. By H. MANNHEIM and L. T. WILKINS. London: H.M.S.O. 1955. Pp. viii+276. 17*s.* 6*d.*

This volume represents 'the first piece of criminological research to be published under the auspices of the Home Office in accordance with the powers given by the Criminal Justice Act, 1948' and is virtually the first comprehensive criminal prediction study done in this country. It is the result of close co-operation between a criminologist and a social statistician supported by the penal authorities. The authors emphasize that 'the combined efforts of administrators and research disciplines' will be essential for further advances in this area. It is thus a landmark in many ways, a piece of research which cannot be ignored by future workers and which has already made considerable impact in Borstal and similar training

fields. Perhaps there is even a danger that it will set the pattern of future investigators too firmly on a narrow basis of replication studies or too rigidly factual, analytic, statistical approaches.

The presentation is both scholarly and workmanlike. The introductory survey in chapter I of American and European prediction studies is exhaustive. Chapter III is a lucid and persuasive piece of teaching explaining the basic ideas underlying prediction, its purposes and uses, even down to a warning that the major value of the study does not lie in the direct application of its experience tables to operational situations but in its demonstration of prediction techniques as research tools. The descriptive analyses and tables of chapter V provide a wealth of information linking all the conventional, mainly sociological data, with themselves and with outcome. One could quibble about some of the minor conclusions drawn here from time to time, especially, for example, where assumptions are made about the 'intentions' or the mental operations of people making ratings or judgements which are later codified. The individual case studies of chapter VIII are interesting but not particularly illuminating. The statistical work (chapters VI and VII and Appendices) is sophisticated and logically developed with efforts to enlighten rather than bewilder the less mathematically inclined. This is especially true of the discussion of the 'decision theory' approach and its implications. Cross-validation is included in the design. A final note (chapter IX) on the future of criminological research will be a source of realistic advice to future investigators on what sort of use can be made of the kinds of data at present in existence and what improvements are necessary here.

I should not regard the main 'practical' findings (that 'open' Borstal treatment is more effective than 'closed', and that length of detention beyond the mean of the study has little effect on success) as so firmly established as the authors claim. Their criticism also of 'subjective' judgements, while not entirely unjustified, appears to me to be unduly harsh. Their plea, however, for further 'operational research' on these and other matters has been convincingly made. This is no doubt an important book which has cleared a way and set a very high standard for future work.

R. L. MORRISON

The Study of Groups. By JOSEPHINE KLEIN. London: Routledge and Kegan Paul. 1956. Pp. ix+200. 21*s*.

Although this book is primarily intended for sociologists, it should be of interest to some psychologists especially to those in applied fields. The material is presented in an attractive and straightforward fashion with very, very few wasted words—a fact that allows the author to bring together in a short book evidence from a large number of researches, ranging from those early rather crude laboratory studies of group strengths in task performances to the recent refined work by Bales, Bavelas and Leavitt and others on social structures and interaction processes. The researches are arranged according to their relevance to certain assumptions made in the text whereby more and more variables are successively allowed to contribute to the increasing complexity of group life as it emerges from the laboratory to real life. The chapters follow this arrangement also, culminating in an ambitious attempt at 'The Larger Society and Social Change'.

The theorist familiar with his bibliography of group studies will not find much new here (except for the Appendix, in which the author relates some of her findings to those of Moreno and Bion) but could regard this work as an excellent refresher. Those whose professional work involves them in group work for the purposes of diagnosis, teaching, or therapy, will undoubtedly find their insights the better from study of this text, not the least for the clear summarizing statements at the end of each chapter.

G. M. STALKER

Discussions on Child Development. Vol. II. Edited by J. M. TANNER and B. INHELDER. London: Tavistock. 1957. Pp. 271. 28*s*.

This volume contains the proceedings of the Second Meeting of the World Health Organization Study Group on the Psychobiological Development of the Child, and continues the stimulating and diversified discussions of volume I (reviewed in this *Journal*, **48**, p. **76**). The broad theme of this volume is 'learning'. As before, one of the participants introduces a topic which is then thrown open for discussion. The topics are: electro-mechanical models of aspects of learning (Grey Walter); effects of perceptual de privation in man and animals (Dalbir Bindra of McGill University); learning under stress in anima

Howard Liddell); the cross-cultural study of learning of internalized standards of behaviour (John Whiting); and the development of abstraction in learning (Bärbel Inhelder): John Bowlby introduces a discussion of the film 'A Two-Year-Old Goes to Hospital', and Konrad Lorenz introduces a film illustrating some behaviour patterns of prematures, newborns, and infants up to about one year of age. As in volume I, the discussion is the important thing. Although this volume may perhaps sparkle less than its predecessor, it is to be recommended as thoroughly worthwhile reading for all psychologists. The constant interplay of queries and suggestions gives the reader the refreshing experience of watching creative thinking in progress, carries him over a wide range of topics, loosens up some of his own preconceptions, and leaves him with plenty to think about. It is one of the book's merits that most readers will want to jump into the discussion with comments of their own.

I. M. L. HUNTER

The Psychology of Adolescence. By A. T. Jersild. New York and London: Macmillan. 1957. Pp. xii+438. 35*s.*

At thirty-five shillings, this book is good value. It may be regarded as a sequel to the same author's well-known Child Psychology; it follows a similar plan and approach. The first five chapters deal with the physical and intellectual growth of the adolescent, often as viewed by the adolescent himself. The remaining twelve chapters deal with various aspects of emotional and social development. It is evident that it is in these aspects that the writer's real interests lie. He does not delve deeply into psychological theory, being content to treat his material at a more descriptive level. Not that the discussion is superficial; the documentation is thorough and up to date, the evidence is drawn from many sources and is allowed to speak for itself with a commentary by the writer, whose insight and maturity of judgement is frequently more illuminating than the evidence itself. This does not claim to be a profound or original study of adolescence. It is 'for those who are seeking to understand adolescents, written for students, teachers and parents...'. It is significant that it is about 'adolescents', not adolescence. Of its class, this book is as good as the best of them, and better than many.

JAMES MAXWELL

The Psychology of Child and Youth Drawing. By Helga Eng. London: Routledge and Kegan Paul. 1957. Pp. viii+205. 50*s.*

It is now twenty-six years since the publication of *The Psychology of Children's Drawings, from the First Stroke to the Coloured Drawing.* Unlike many children, Margaret continued to draw throughout adolescence, and in this book Dr Eng traces her artistic development from the ninth to the twenty-fourth year. The first ten chapters contain 114 drawings and paintings, with descriptive text, and the remaining eighteen summarize the work done in this much-explored field during the past seventy years. There is a full and useful bibliography.

One cannot help questioning some of Dr Eng's premises: for example, that children's drawing, uninfluenced by formal instruction, is necessarily 'free'; that subjects such as elves and fairies are evidence of imagination; and that copying is always conscious. It is just at the self-critical stage that Margaret has reached at the beginning of the book that, without suitable stimulation and guidance, copying is most likely to occur. This may account for the difficulty Dr Eng meets when she tries to relate the drawings to the theories of Herbert Read and Viktor Lowenfeld. The book contains at the same time too much and too little information. A full psychological study of Margaret, illustrated by her drawings, or an integrated account of work done in this field, would have provided a more useful contribution to an understanding of child art.

JOYCE COOKSON

Psychology in the Classroom. By James Mainwaring. London: University of London Press. 1956. Pp. x+118. 7*s.* 6*d.*

This book is addressed to students and teachers but one cannot help thinking that the latter group will know much of what it contains already and will have little interest in the rest!

The book is split into four sections. The sections on 'Learning' and 'Knowing at the moment', base their theoretical matter on a highly dubious model of Dr Mainwaring's own invention. In consequence, the practical illustrations in both sections are more readily digestible than are the abstract formulations.

The third section is limited to a consideration of a few of those problems associated with thinking. Th aspects discussed are explained lucidly and in a more usual and acceptable theoretical framework. Th final section on 'Feeling' is more philosophical than psychological, which perhaps explains why so litt is said about individual differences. The old thorny problem of discipline, left to the last, rates a chapte to itself. This chapter provides a worthwhile text for any prospective primary teacher. Unfortunately i does little to help the poor harassed secondary teacher, who has to deal with children who had develope under a system far removed from the author's!

At times the terminology is muddling, as, for example, when the term 'physical response' is used t denote the sensory stimulus, and, throughout the book, far too little reference is made to the experimenta aspects of educational psychology. Despite these limitations and the inadequacies on the theoretica side, this book should be very useful in introducing Training College students to the subject.

G. SHOUKSMIT

A Case of Spatial Inability in a Cerebral Palsied Child. By E. M. Caldwell. London British Council for the Welfare of Spastics. 1956. Pp. v+24. 4*s.* 6*d.*

This booklet is written by a teacher for those concerned with the training of children suffering fro cerebral palsy. The term 'spatial inability' is given to a syndrome which, in adults, is differentiated b neurologists into visuo-spatial disorientation, disorders of body-image, and the various types of agnosi apraxia, aphasia, and acalculia. The disabilities are illustrated by an account of the progress of one chil over a period of about 15 months and by a table giving brief notes of findings on thirty-three children age from 6·6 to 13·4 in the Percy Hedley School at Newcastle. The various suggestions for special forms instruction and for modification of the usual methods will be useful to teachers and therapists dealin with such children. It is to be hoped that the psychologist who provided the test results will be give space to elucidate and amplify his findings in the more detailed report promised for a later date.

J. A. L. NAUGHTO

Clinical Psychology: The Study of Persons. By Richard W. Wallen. London: McGraw Hill. 1956. Pp. 388. 45*s.*

This work is intended as a text-book for a first course in clinical psychology, and to be suitable for eith advanced undergraduates, or the first year of a graduate course. A background of knowledge in t psychology of personality, normal and abnormal, and in psychological 'testing', is assumed. The author theoretical position, he considers, 'leans toward neo-behaviorism': in practice, he adopts a reasonab eclecticism. Among the topics singled out for special consideration are: prediction in clinical psychology clinical sensitivity and hypothesis making, with remarks on the blunting of sensitivity by some types academic training, masquerading as scientific objectivity, and on excessive diffidence in the use simple observation; interviewing; the use of tests, psychometric and projective, with special mentio of Rorschach and T.A.T.; and psychotherapy. There is also a chapter on medical assessment, whic attempts to cover too much in its space.

The book is naturally orientated towards the North American scene. In general, this is of importan only in relation to the practical illustrations, e.g., the line of inquiry which gets from 'dates' to 'clap' five easy moves might strike the Briton as somewhat brusque. The necessary transpositions can readily b made by anyone interested in the subject.

The book is evidently the product of a teacher as well as a clinician, and takes this aspect of its functio seriously. The text is frequently interspersed with problems related to the preceding subject-matte rather to encourage speculation and an interpretive attitude, than just as a check on absorption-rate.

The limitations of reading as an introduction to this type of work are, however, clearly recognized.

It is encouraging that a clinical psychology can be taught, which is not amateur psychiatry, simp psychometrics, nor merely a pretext for the use of clinical material to prosecute theoretical inquirie and which is soundly based scientifically but keeps a firm grasp of practical exigencies.

J. A. L. NAUGHTO

Physiological Psychology. By M. A. Wenger, F. N. Jones and M. H. Jones. New York: Holt; London: Constable. 1956. Pp. viii+472. 40*s*.

Intended as an introductory text, this work is, with some reservations, as good as one could reasonably expect to find in a field as broad as physiological psychology. Such books cannot be easy to write, if only because the authors are bound to lay themselves open to criticism regarding the selection of material. Certainly, in this particular case, it seems strange to find references to the work of, for example, Békésy, Galambos, Harlow, Magoun, Penfield and Selye, but no mention of, for example, Granit, Hunt, Lashley, Motokawa, Neff and Pribram. Given the interests of the senior author, it is understandable that the sections on autonomic functions and on psychosomatic inter-relations are better written than the remainder of the book (incidentally, is the term 'somatopsychics' really necessary?). The last chapter, in which the authors come to regard the preceding two dozen chapters as a 'survey of facts and principles', is supposed to highlight the issues currently important in research, but in fact has been limited in scope mainly to consideration of the subject-matter of the Milbank Memorial Fund's 1952 Symposium on the Biology of Mental Health and Disease. The book is adorned with two excellent diagrams from the Ciba Collection of Medical Illustrations, and a reasonably clear glossary of terms, but has an irritating reference system. 'Suggested readings' at the end of each chapter are supplemented by more extensive lists of sources at the end of the book, but these are arranged, again, chapter by chapter, and occasionally include such unhelpful entries as, for example, 'Wenger, M.A., Unpublished research' (p. 445)—this particular entry never being actually referred to in chapter 17. A few other entries, although referred to in the text, prove wrong on further inquiry (e.g. on p. 280 Ref. 8 should read Ref. 7). Misprints such as 'recticular formation' (p. 252) are inexcusable.

S. SZAFRAN

Acta Physiologica Scandinavica. Vol. 39, Supplement 134. Stockholm. 1956. Pp. 112. $3.00.

This volume comprises six papers. The first, by G. Svaetichin and R. Jonasson, is 'A technique for oscillographic recording of spectral response curves'. The remaining papers, by Svaetichin, are 'Spectral response curves from single cones', 'Receptor mechanisms for flicker and fusion', 'Notes on ERG analysis', 'The cone function related to the activity of retinal neurones', and 'Aspects on human photoreceptor mechanisms'. It is of particular interest that the authors have obtained spectral response curves by the stimulation of single cones in the retinae of Teleost fish in Venezuela. They used twenty-four almost monochromatic interference filters distributed over the visible spectrum as light sources. These fish have colour vision essentially the same as that of man. The experimental data give very strong support to a four-colour theory of colour vision not at all unlike that outlined by Houstoun in 1932, and there is strong evidence that brightness or subjective luminosity may be mediated in part by a special type of cone. Three types of response patterns were obtained from different cones, and not more than three. The first shows depolarization in the red region of the spectrum and hyperpolarization in the green; the second shows depolarization in the yellow and hyperpolarization in the blue; and the third shows depolarization in proportion to the luminosity of the whole spectrum. In Teleost fish, and in some diurnal vertebrates which have colour vision, some cones are large and single, while others are smaller and double. Svaetichin has suggested that the large single cones may give the response signalling luminosity, while the small double cones may be of two kinds, each kind giving alternative responses, one for red or yellow and the other for green or blue, according to the kind of double cone in action. No double cone can give both the red and green or both the yellow and blue signals at the same time (in normal colour vision). Svaetichin has suggested that the wave-lengths of the lights giving peak responses for each pair might be as follows for man, the maxima for fish being shifted 20 mμ to the short wave end of the spectrum: red, about 634 and 420 mμ; green, about 486 mμ; yellow about 590 mμ; blue about 440 mμ. The maximum for luminosity would be about 554 mμ. These peaks for colour would correspond to the points of maximum hue discrimination for man. Achromatic subjects might lack all but the luminosity receptors.

If experiments such as Svaetichin and Jonasson have reported could be carried out on the human retina, and this is probably not an impossibility, much could be done to test four-colour theories, which appear to be strongly supported by observations on colour blindness, anomalous colour vision, and minor variations in colour sensitivity reported by various authors. Judging by experiments on human colour

vision and its variability, the peaks for sepctral response curves for man would be about 671 mμ for red (with an unrevealed peak in the violet), 546 mμ for green, 593 mμ for yellow and 488 mμ for blue. These differ somewhat from the peaks suggested by Svaetichin, but the differences might be due to various factors, including the special ways in which the response curves combined in actual vision, and the effects of pigmentation of the transparent parts of the optical system.

R. W. PICKFORD

Research Films in Biology, Anthropology, Psychology and Medicine. By A. R. Michaelis. New York: Academic Press. 1955. Pp. xvi+490. $10.00.

Dr Michaelis, by training a chemist and by profession a consultant in scientific cinematography, has, we are told, 'made research films on numerous subjects, including the circulation of blood in frogs, the spinning of aeronautical models in wind-tunnels, and the behaviour of bubbles in liquids'. This is his guide book, specially written for those who would make or use scientific films of a biological, anthropological, psychological, or medical nature. It should be read by anyone contemplating using a ciné camera before, if possible, the camera is bought, since it will help him to make a wise purchase. To those who have a camera, it will suggest new uses and improved techniques: to those who but use films it supplies a background of knowledge which should help in the critical evaluation of what is presented.

Following a general introduction of 31 pages, Part I (129 pages) deals with 'The Biological Sciences'. Fifteen of these pages are devoted to animal behaviour. These include references to the work of Von Frisch, Lorenz, Köhler, Schlosberg and Katz, Liddell, Yerkes, Pavlov, Masserman and Mowrer. Part II (99 pages) is concerned with 'The Human Sciences', Anthropology, Psychology and Psychiatry. Twenty-five pages are devoted to Psychology, as here defined, with over a hundred references to films in Child, Educational, General and Industrial Psychology. Part III (101 pages) refers to 'the Medical Sciences'. Here we learn how the film has been used in the study of locomotion, digestion, respiration, etc. A final chapter deals with Aviation Medicine. Each part has its own special introduction devoted to specialized techniques. For example, endoscopic cinematography is discussed in the introduction to Part III. The book concludes with an excellent Author and Reference Index (91 pages with 1490 references) and a good Subject Index.

The division of the subject-matter implied by the titles to Parts I, II, and III is open to objection, especially since it may create the impression that this book is of less importance to the psychologist than in fact it is. I have called this a guide book, which it is, but I must add that it displays a degree of scientific detachment hardly characteristic of such books. Nothing is advertised: certain apparatus and methods are advocated. This book merits a place in every Psychology Department.

HENRY H. FERGUSON

Les Structures Rhythmiques. By Paul Fraisse. Publications Universitaires de Louvain. 1956. Pp. viii+124. No price given.

The neglect of this particular field of work during recent years is no doubt largely due to the difficulty of devising procedures which can effectively investigate rhythm. Prof. Fraisse is all the more to be congratulated for tackling the problem and for his very thorough analysis of the conditions and limits of rhythmic structure. The first experiments grew out of the work directed by Prof. Michotte at Louvain on the 'morphology' of motor reactions and the present book gives the results of twenty years work on the problem.

Periodicity and structure are both inherent in our experience of rhythm and there is a motor component which makes it an experience rather than a perception. The conditions influencing rhythmic grouping include: length of intervals, duration of sounds, accentuation, speed, and number of elements. Rhythmic structure has a temporal aspect, which assures the unity of the group, and an intensive aspect which appears only with the repetition of the group. The temporal structure turns out to be astonishingly consistent; the groupings make use of two intervals, a 'short' and a 'long', whose actual durations are of the order of 0·2+ sec. and 0·5+ sec.; the ratios between the various intervals in a group are mainly of the order of 1 and 2. Reproductions of groupings which do not make use of 'pregnant' forms involving simple and double ratios between intervals show the play of principles of 'assimilation' and 'distinction' which modify in the direction of pregnancy. Modifications tend to affect the 'long' interval, suggesting that the 'short' is more fundamental. It is not only the relationships within the group which are important; rhythm is not to be considered as repetition of isolated groups, but the experience depends on organization of successive stimuli which also takes into account the length of the pause between groups

Looking for a final interpretation in terms of physiology, Fraisse distinguishes 'perception of collection' and 'perception of duration'; taking the duration of the perceptual process to be about 0·5 sec., perception of collection ('short intervals') occurs if intervals are less than 0·5 sec. and perception of duration ('long' intervals and pauses) occurs if the intervals are greater than 0·5 sec. 'Long' intervals and pauses are distinguished by being 'full' or 'empty' but the physiological basis of this distinction does not seem to be so well established.

Intensive structure is discussed briefly and the author himself admits that he has not offered very full support for his hypotheses. The evidence so far available, however, seems to justify the belief that temporal structure is the more fundamental.

The book concludes with a consideration of music and poetry in relation to the laboratory studies. The significance of the experimental results is clear and further analyses would be welcome in order to bridge fully the gap between the simplicity of the experimental rhythms and the complexity of music and language.

MARGARET R. PHEMISTER

The Psychology of Learning. By B. R. Bugelski. New York: Henry Holt; London: Methuen. 1956. Pp. xiii + 523. 50*s.*

There seem to be two obvious ways of planning a text-book on learning. Either we take it personality by personality and treat each theory as its proud parent would have us see it, or we can cover the ground topic by topic letting each contender have his say on each issue. Anyone nurtured on Hilgard will tend to overlook the fact that who-said-what-when is not necessarily still of interest when we come to assess the present status of learning theory.

As Bugelski, with what sounds like a tone of faint exasperation, puts it: 'The question of the variety or number of kinds of learning appears to be a pseudo-question'. This is a provocative thought, and typical of the panache with which he tackles the whole problem area. He picks his way through the tangle of conflicting viewpoints with an impressive sureness of foot. For the most part, it reads like a good lecture—the plan is not so obvious as to invite tedium, but everything seems to follow logically enough. The most recent literature has been combed with a good eye for the telling experiment and each reference has a way of popping up in just the right place.

Teachers who, since 1953, have wearied of the student whose standard plonking tactic is 'What about Osgood's mediation hypothesis?' will thank Bugelski for an account of mediated behaviour which is both clear and adequate. If there is to be a criticism of this book, it might be said that Bugelski is a little too fond of Hebb's brand of meta-neurology. Since Hebb has already warned us that he expects to find his views not more than 95% mistaken, Bugelski might have added a more cautionary note to the brilliant speculation. But, like it or lump it, there is little doubt that 'The Psychology of Learning' will be with us for the next few years.

R. T. GREEN

The Mentality of Apes. By Wolfgang Köhler. Harmondsworth: Penguin. 1957. Pp. 286. 3*s.* 6*d.*

This inexpensive and very welcome volume is a reprinting of Köhler's classical monograph as it appeared in English in 1925. The nine photographic plates of the 1925 edition are omitted but, otherwise, no thing has been added or subtracted.

I. M. L. HUNTER

The Unconscious Motives of War. By Alix Strachey. London: George Allen and Unwin. 1957. Pp. 283. 25*s.*

As would be expected from its author, this book contains a very clear and illuminating account of the basic ideas and themes of psycho-analysis. In the first part, ten chapters are devoted to the instincts, ego-functions and defences, to the origins of the moral sense, the stages of infantile development, dreams and regressive phenomena. It is useful that in the chapter on the perils of regression Mrs Strachey makes a serious attempt to deal with psychotic problems, because they are specially relevant to war. In the second part three chapters are devoted to the group influences underlying war. The social group is viewed as a regressive influence, at least in respect of its bearings on war, and the special dangers of the sovereign state as an institution eminently suited to act as a regressive group are well expressed.

In the third part various recommendations are made by which tensions leading to war might be reduced. These are divided into three groups, the first of which includes changes towards a reduction of authoritarianism and dictatorial control in the state. The second includes changes to be brought about in the individual by decreasing his frustrations and traumata in early infancy as much as possible, and thus establishing a healthily balanced attitude to adult life, not over-subject to group-proneness. The author does not underestimate the difficulties of reducing infantile traumata and frustrations. The last group of recommendations is about the value of psychoanalysis itself, and this leads to the concluding chapter in which Mrs Strachey suggests that psychoanalysis might be applied with advantage to people in five categories as a prophylactic measure against war. These categories are: persons more than ordinarily hostile, aggressive and destructive; persons who have a marked instinct of domination; persons in positions of authority; persons who happen to be highly charged with State-mindedness; and those who are exceptionally group-prone.

Possibly the weakest aspect of this admirable book is that the author may underestimate the resistance which would be offered to her recommendations, valuable though they would be, especially because the major tensions leading to war are probably of a psychotic nature and less accessible to psychoanalytic amelioration than we could wish.

R. W. PICKFORD

Towards a measure of Man. By PAUL HALMOS. London: Routledge and Kegan Paul. 1957. Pp. viii+250. 28*s*.

This book consists of a string of essays in which the writer attempts to define the concepts of normality and abnormality in human behaviour and personality. Halmos attempts to educe 'an absolute measure of man', and he does so by relying, as he says, 'on metaphysical, biological, and psychological information'. It is no accident that these sources of information are listed in this particular order, and the reader should be warned that when Halmos says 'psychological' he in fact means 'psychoanalytical'.

Those who like speculative thinking, subjective arguments, and a freedom from the discipline which experimental facts enforce, will probably enjoy this book. Readers of a more empirical bent will not so readily acquiesce in the easy acceptance of Freudian dogma and the failure to survey the facts relevant to his thesis characteristic of Halmos's book. To take but one example, Halmos quotes with approval Bowlby's doctrine of mother-child separation on p. 63 of his book, but he fails to give a critical summary of the research literature which would show that this doctrine is extremely insecurely founded and probably erroneous. Halmos would probably argue that this was meant merely as an illustration, but a whole book built up on illustrations in disregard of established fact cannot be unreservedly welcomed.

In many ways it is a pity that the book is disfigured in these ways, because Halmos's objective is an important and a sensible one. A thorough review of the facts of adjustment and the conditions relating thereto would be of great value and could easily be fitted into the kind of philosophical framework Halmos has in mind. Such a book would have made a genuine contribution to science. As it is, *Towards a Measure of Man* is more likely to find its place in the category of Belles Lettres.

H. J EYSENCK

The Person in Psychology: Reality or Abstraction. By PAUL LAFITTE. London: Routledge and Kegan Paul. 1957. Pp. x+233. 25*s*.

The book—states Dr Lafitte in the Preface—is 'a general argument about the nature of psychology'. Psychology is said to be in a bad way. Its weaknesses are: 'insufficient facts, sharply conflicting interpretations, neglect of the study of the greater part of common experience and a wide gap between the person of professional psychology and the person as commonly known' (p. 4). All these will not be remedied by studying the person in an objective manner: the study of neurology and the study of animal behaviour are, for the most part, irrelevant to psychology at large; objective test measurements are hardly better.

To remedy its weaknesses, psychology should return 'to an open dependence on common experience' (p. 47). To the extent that social psychology does this, it is exempt from the severe strictures made upon other kinds of psychology. Psychological inquiry 'must include the detailed study of wide and complex areas of ordinary behaviour' (p. 100). When studying a person some 'short cuts' are available. One is 'substantial abstraction', so called because it seeks the substance of the person's recurrent behaviour; it is based on information ranging from 'ordinary sustained acquaintance' to various projective techniques. There are others called 'instrumental definition' and 'synthetic abstraction'; the

efer to the ways of gathering and classifying data. A full investigation includes also the person's history, nd his exceptional (non-recurrent) behaviour. All these methods of inquiry are amply illustrated by udies carried out by the author and by his colleagues at the University of Melbourne.

Methodological considerations loom large in the book, and the author attaches a great deal of impor- ance to them. Comparisons are drawn between psychology and other disciplines. The nature of psycho- gical laws is discussed at some length. Philosophers of science would probably find all this rather con- oversial ground.

The interest of psychologists in methodology is understandable. There has been a lot of discussion as o what psychology should be like. Such discussion implies a peculiar unity of the very many different inds of studies that psychologists undertake. The nature or degree of this unity may be questioned. lowever, from the standpoint of the advancement of knowledge, discussions by psychologists of the erits and demerits of particular studies may well be more valuable than discussions of general ethodology.

W. SLUCKIN

'Instinct dans le Comportement des Animaux et de L'Homme. By various authors. Paris: Masson. 1956. Pp. 796. Paper 6.500 fr.; cloth 7.200 fr.

n 1954, the Fondation Singer-Polignac organized a symposium on 'Instinct'. The fourteen participants om the European continent included K. von Frisch, H. Hediger, O. Koehler and K. Lorenz; the merican members were R. Gesell, D. Lehrman, C. Richter, T. Schneirla and (from Brazil) M. Autuori; ne Britishers were J. B. S. Haldane, Mrs Haldane-Spurway and D. Morris. This substantial volume ontains the twenty-one papers presented at the symposium (eleven in French, seven in English, three German), along with transcripts of the discussions following each paper, an introduction by P. Grassé, nd a postscript by Henri Piéron. The greater part of the volume is devoted to animal studies of the ind which have come to be known as ethology; but there are excursions into the realms of biochemistry nd endocrinology, the physiology of the nervous system and of metabolism, and psychoanalysis and hilosophy. It is impossible here, to list the titles of the papers, much less comment on them, but this an important source-book for psychologists who are interested in animal behaviour and in the physio- gical and evolutionary aspects of their discipline. And for the theoretical psychologist it is a forceful minder that there exists no easy answer to the question 'what is instinct?' The discussions make it ear that behaviour cannot be dichotomized into innate and acquired. As used in this symposium, nstinct' is but a chapter heading in biology containing many subsections also called instinctive and ill to be precisely distinguished from one another. But the richness of the research findings presented ows that ambiguity and even disagreement over the word 'instinct' need be no deterrent either to mmunication or to rewarding empirical investigation.

I. M. L. HUNTER

ysergic Acid Diethylamide and Mescaline in Experimental Psychiatry. Louis Cholden (Ed.). New York and London: Grune and Stratton. 1956. Pp. xi+85. $3.00.

Drug-taker's Notes. By R. H. Ward. London: Gollancz. 1957. Pp. 222. 16*s*.

esearch into the problem of psychosis is proceeding vigorously. The Symposium of the American sychiatric Association, edited by Cholden, is perhaps the most significant publication to appear since ne new discoveries of the Osmond, Smythies and Hoffer research team (1952, 1954). This book explores theory: that the basis of the functional psychoses is biochemical; and relates to an accompanying search technique: the experimental production of altered mental states, 'model psychoses', by adminis- ation of hallucinogen drugs such as mescaline, lysergic acid diethylamide (LSD) and adrenochrome. uch a theory and technique logically imply the research collaboration of psychiatrist, psychologist and ochemist. In the Symposium, Rinkel (psychiatrist) discusses the Osmond–Smythies theory that some bstance of the mescaline-LSD group is a necessary causal factor in functional psychosis, and the later offer–Osmond–Smythies theory that adrenochrome may be this substance. Rothlin (pharmacologist), irector of the Sandoz Laboratory where the hallucinogenic properties of LSD were discovered, gives a mprehensive account of this drug's pharmacology. Among other contributions is Hoffer's (psychiatrist) count of experiments showing the inability of psychiatrist observers to discriminate between psychotic tients and model psychosis subjects to whom LSD has been administered, from sound-recorded inter- ews. Those who would stress social rather than chemical factors in the aetiology of psychosis will be

interested in the contribution of Savage (psychiatrist) who, while accepting a biochemical causal proce discusses how to avoid mistakes in handling schizophrenics of a kind he believes are commonly made the community and therapist alike.

Criticism must be made of the thinness of references to earlier investigators: Prentiss and Morg Havelock Ellis, Weir Mitchell, Beringer and Mayer Gross, Klüver, and others. For readers of this *Jour* a serious defect is the indifferent quality of the two papers which deal with the psychological side mescaline and LSD reactions. In the first of these Sandison (psychiatrist) argues that hallucinogen dr help in the understanding of neurosis as well as psychosis; this claim is advanced with a great deal m dogmatism than evidence. Sandison's account gains nothing from his use of concepts as diffuse as 'bi fantasies' and Jungian 'mandala symbols' when he might instead have chosen to refer to the care work of Klüver on 'form constants' in both natural and experimentally produced hallucination.

The second of these two papers, by Aldous Huxley ('Man of letters') is of such poor quality that i difficult to understand its inclusion. Huxley chooses repeatedly to ignore individually different reacti to mescaline. His concepts fall short of even minimal criteria of precision, and fanciful metaphors l 'the mind's antipodes' and 'the psychological equivalent of Australia', together with dubious generali tions on such themes as 'the almost universal passion for flowers' do not strengthen this as an object communication on the psychological effects of mescaline. This contributor's over-generalizations ab psychosis, mysticism and human nature in general on the basis of what appears to have been a sin mesculine experiment on himself do not escape censure even within the Symposium itself. As Fis remarks: 'I think that from Mr Huxley's mescaline experience there is 99 per cent Aldous Huxley a only one half gram mescaline' (p. 67). It is unfortunate for serious research in this field that his o mescaline experiment—about which Mr Huxley has already written two books—has been so mu popularized by Mr Huxley and with the implicit assumption that everybody reacts in a standardi way; perhaps it is a pity that Huxley's inclusion in the Symposium has given another opportunity these gross over-generalizations to be repeated. Yet it is to be hoped that this slim but important bo will, taken as a whole, do something to attract both lay and specialist attention to the more major iss raised by the model psychosis experiment.

Mr Ward's account of a series of experiments with lysergic acid diethylamide on himself is a be book than its title, or the publisher's blurb on its dust cover, would suggest. It is well written, the experiments in question being reported in a lucid and interesting way; Ward exhibits careful attentio reports by earlier investigators, insight in describing what happened to him personally, and caut about attributing his own experiences to everybody. The dust cover is unworthy of the book whe poses the question: 'Who is right, Mr Ward or Mr Huxley?' Since Mr Huxley took *mescaline* (on a sin occasion) and Mr Ward took *lysergic acid* (in six experiments) it is difficult to attach much meanin this question. If mescaline is to be mentioned at all then Mr Ward's careful observations on himself v LSD merit comparison with the best of the accounts of mescaline experiences, e.g. that of Weir Mitc in 1896. Moreover, Mr Ward's attitude not only to the possibility of different individual reactions to same drug, but also to different reactions of the same individual on different occasions is highly soph cated and, in the reviewer's opinion, eminently sound. Because of this quality, and because he careful introspectionist who is also a good writer, Mr Ward's reports are of considerable interest to psychology of the LSD reaction. Readers of this book will gain from it some understanding of serious purpose behind model psychosis experiments which have continued since Prentiss and Mor late last century, why they are proceeding vigorously at the present moment, and what it is all ab

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